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EGG-WEIGHT AS A CRITERION OF NUMERICAL PRODUCTION IN THE DOMESTIC FOWL[1]

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I

WHEN one surveys the field of literature dedicated to the subject of egg-production in the domestic fowl he may well be astonished at the vast number of ways and means by which a poultryman can detect the best layers of his flock. Indeed one exaggerates only slightly to say that there is scarcely an incident in the hen's daily program, nor an event in her life, that has not been interpreted by some unusually keen observer as a sign of producing ability—good or poor. Was the hen seen to rise early and dispatch a one-hundred-calory portion of mash, together with nine bugs and three worms, before her sisters were off the roost? Then put her down unqualifiedly as an industrious hen and enthusiastic layer—a credit to any poultry house. Was the hen observed to work after hours gleaning the last fragment of grain from the litter, or perchance chasing lightning bugs through the twilight grass, when other union-members of the flock had ceased work for the day and retired to roost? Then register her as one that has her master's interests at heart, and one that should be vigorously encouraged to reproduce her like. Did the hen lose the yellow glamour of her shanks and beak (doubtless the equivalent of good complexion in a hen)? Did she molt in July or August? Was her comb

[1] Contribution 250 from the Agricultural Experiment Station of the Rhode Island State College.

a ruddy red in September? Was her pelvis broad and flexible in April? Did she start laying in October? Did she lay thirty or more eggs before the first day of March? Did she lay 200 eggs in her pullet year, or 500 eggs in three years? Did she lay small eggs or large eggs? By all these signs one may (it is alleged) detect the hen that is (or has been) the good producer. But the curious part of the matter is that, notwithstanding these many signs and evidences of producing ability, the hens of the average poultryman continue to deliver the same number of eggs per year—estimated at about 120.

Among this variety of criteria, however, it must in fairness be said that some of the tests are of practical significance. It can scarcely be doubted that, as a rule, hens that lay the largest number of eggs during the "winter period" (November 1 to March 1), as first stated by Pearl, are the best layers for the entire year. On the other hand, it has been shown by Goodale that the production during the winter period may be strongly influenced by the time of hatching: the early-hatched hens make the highest winter records—at least they lay the greater number of eggs between the beginning of the laying period (sometimes as early as August) and March 1.[2]

If a hen is entitled to be called a good producer only on condition that she makes a creditable record for two or more years successively—then there is point to the recent contention of other investigators that hens that make a low first year's record usually "make up" during the second year, so that a three-year production record appears to them as representing the fairest measure of producing ability. This is of course the equivalent of saying that the number of eggs that a hen lays is a good criterion of her egg-producing ability—a circumstance which no one can deny. But it frequently happens that, for practical purposes, one desires such a criterion as will indi-

[2] It may be a question, however, whether the "winter period" of Goodale's early-hatched pullets may not in reality represent a combining of *two* laying cycles. His data on production seem to make possible this interpretation.

cate a hen's producing ability before she has attained that stage in life when economic production ceases; and when, even as a breeder, her further producing days are few.

II

To the casual reader it will no doubt appear preposterous that a biologist should attempt to measure the numerical egg-production of a hen by *weighing* her eggs, rather than by counting them. But the author freely admits that this ridiculous thing has actually been done in his laboratories; and, what is more, that the method appears to work: a flock of hens can be divided into groups, each characterized by a different mean producing ability, as a result of weighing a certain number of eggs at a certain time in the laying year, and subsequently by making certain computations therefrom. The results depend upon *the relation existing between egg-weight and egg-production at different periods of the laying year.* These points may be considered separately.

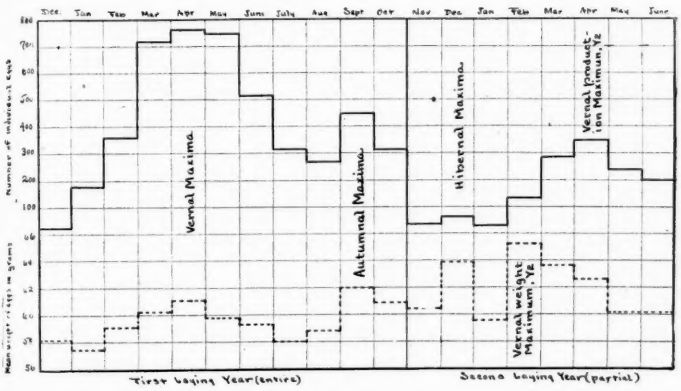

FIG. 1.

When the first yearly production of a flock of hens of equal age and condition is plotted on monthly ordinates one frequently obtains a curve such as indicated by the full line in Fig. 1. It is observed that the production

curve for the year ending October 31 is of the bimodal type.[3]

One of these modes, appearing on the spring of the year (in April), may be termed the *vernal production maximum.* The second mode, falling in September, may be called the *autumnal production maximum.* Of these two maxima for the first laying year the vernal maximum manifests the higher peak, in keeping with the heavy April production which is the highest of the year. Between any two successive maxima lies a fundus whose minimum is attained either in July or August, or in November of each year. The late summer depression may be termed the *æstival production minimum,* and the November depression, the *autumnal production minimum.* In the month of December of the second laying year it will be noted that the curve rises slightly. This represents the increased production of the "winter cycle" or, as it may be called, the *hibernal production maximum.* Between this and the vernal production maximum of the second laying year is another depression in the curve, following the period of winter production and indicating the *hibernal production minimum.* This is followed in turn by the vernal maximum of the second year.

The presence of these two maxima in the curve of production for the first laying year indicates that at least twice during this year (closing October 31), once in the spring and once in the autumn, the egg production of a hen rises from its lower limits and expresses itself by laying a larger number of eggs than at any other season. These periods of increased production represent the spring and the autumn cycles respectively. There exists also the winter cycle which is usually manifested with clearness only in those flocks which show a fairly high mean production. It is a significant productive period, but will not receive further consideration at this time. It may be added, however, that in birds that are fairly high producers and which are hatched sufficiently early

[3] These hens were poor winter producers and the expected mode for December-January of the pullet year does not appear.

in the year, the winter production maximum may make its appearance as the first cycle of production of the first laying year.

We have considered some of the more obvious variations in the curve of numerical production and come now to the curve of variations in egg-weight. Such variations may be considered with reference to the clutch, the litter, the cycle or the year.[4]

For present convenience, however, we shall consider the variation by months—purely arbitrary divisions in the life of the hen, which cut in on, and interrupt clutches, litters and cycles in such a way as frequently to obscure many of the problems involved. For our present purpose, however, division by months offers a rough and ready division of the year into short periods of time in which the productions may be compared.

When all the eggs laid by a flock of hens are weighed and recorded, and the monthly means computed and plotted on monthly ordinates, such a curve of mean monthly variation in egg-weight is obtained as that shown by the broken line in Fig. 1. Such a curve shows that all the eggs that a hen lays are by no means of equal weight. The first eggs laid are relatively small, but the weight increases gradually until a maximum weight for the first year is attained in the month of April. This is termed the *vernal weight maximum* and may represent mean increase of five grams over the mean weight of eggs for the first laying-month of the year. This maximum forms the first mode of the frequency curve of variation in egg weight as shown in the figure.

After April, the curve of variation in egg-weight drops for May, again for June, and reaches the lowest point in July, at which time the mean weight of the eggs of the flock may be scarcely greater than for the first month of production. Having struck this low point, however, the

[4] A *clutch* may be regarded as the group of eggs laid on successive days without an interruption. A *litter* is the group of eggs laid immediately preceding the onset of a broody period. A *cycle* is the larger group of eggs laid during any one of the seasonal periods of increased production.

mean egg-weight begins to rise again and reaches a second mode or maximum in September, at which peak the mean egg weight is slightly higher than for the period of the vernal maximum (April). This September peak may be called the *autumnal weight maximum.* The mean difference between the vernal maximum and the autumnal maximum is usually about one or two grams. Having attained this peak of weight, the curve drops again through October to strike its fundus in November (the first month of the second laying-year).[5] From this point it rises in December to the first weight maximum (*hibernal weight maximum*) of the second laying year, and then drops again in January to form the *hibernal weight minimum* immediately preceding the vernal maximum of the second year.

It will now be clear to the reader that there exists a noteworthy circumstance with reference to these curves of numerical production and of egg-weight: they parallel one another to a remarkable degree. The vernal maxima of production and of weight fall together in April; and the autumnal maxima of production and of weight fall together in September. The only departure from coincidence lies in the circumstance that the summer production minimum arrives in August, while the summer weight minimum is found in July. It should be said, however, that the plotting of the curves on ten or five-day ordinates might show a closer correspondence of these minima in point of time. The difference observed is scarcely significant. The definite agreements in the trends of the respective curves are taken to indicate that, on the average, increased production is accompanied by increased mean weight of the eggs produced; and that, *vice versa,* a decrease in production is, on the average, accompanied by decreased mean weight in the eggs produced. Whatever, therefore, may be the biological significance of the two production maxima for the hen's first

[5] It has become common to consider the laying year of a hen as extending from November 1 of the pullet year to and including October 31 of the year following.

laying year, the weight maxima would appear to possess a similar significance. Since the two are so closely correlated it would seem possible to measure a hen's innate egg-producing ability by the one phenomenon as well as by the other. This constitutes the hypothesis which we will now attempt to verify.

III

If we take a cross-section of the April production as nearly as possible to the absolute mode[6] of the weight curve, we learn that, although the egg-weight of most of the individuals of the flock has increased at this time, there are a few in which it has not increased significantly; and a still smaller number in which there has occurred a loss in egg-weight. The same is true for a cross-section of production taken at or near the absolute mode of the autumnal weight maximum. The following question therefore arises: *Does there exist any significant correlation between a tendency to manifest an increase in egg-weight at the period of the vernal weight maximum (or autumnal weight maximum) and the number of eggs produced for the entire first laying year (November 1 to October 31 following)?*

In order to demonstrate such a correlation one must first define more exactly the nature of the second variable, namely, the "tendency to manifest increased egg-weight" as referred to above. There must be a fixed point from which one can calculate, for each individual hen, the amount or the extent of increase in egg-weight manifested at the weight maxima. For certain reasons

[6] In explanation of this point it may be added that by plotting the frequency distribution of variation in April egg-weight on daily ordinates one may approximate more closely the absolute mode. This has been found to lie (for the flock in question) between April 11 and 15. For the autumnal weight maximum it lies between September 21 and 25. The absolute vernal production maximum lies between April 16 and 20, while the absolute autumnal production maximum appears between September 11 and 15. It is of course to be expected that these dates are only relative; that they would vary with different flocks, depending upon the climate, the date of hatching, the method of housing and presumably upon still other varying, environmental factors.

it was decided to compute all increase or decrease in egg-weight, for each individual, from the mean weight of the first ten eggs laid at the beginning of the first laying year of that particular bird. And, in order to translate the differences into comparable terms, the increase or decrease was calculated as a percentage increase or as a percentage decrease above or below the mean weight of those first ten eggs. Consequently, the percentage of increase or decrease in mean weight for all April eggs, over or under the mean weight of the first ten eggs laid, was ascertained in the case of each bird in the flock; and the same data were derived for the September production. It is upon the analysis of these raw data[7] that the appended computations rest. In the succeeding paragraphs it is therefore the aim of the writer to demonstrate the following point: that the higher percentages of increase in mean egg-weight, reckoned at the periods of the weight maxima, are so closely correlated with higher production for the first laying year, that, by the method to be presented, a flock of hens may be divided into groups characterized respectively by higher, medium and lower producing ability; and that this method is effective, whether the computations are based upon the vernal or the autumnal weight maxima.

IV

We may first concern ourselves with computations based upon the mean weight of the April eggs, including the eggs of the entire month; and it is scarcely necessary to resort to formal correlation tables to demonstrate the point involved. The simpler methods may be employed: (1) Dividing the birds on the basis of annual production above or below the flock-mean and then computing the percentage of net increase or decrease in mean egg-weight; (2) dividing the birds into groups based upon percentages of net increase or decrease in mean egg-

[7] It would be impossible to present these raw data in an article of this scope. They will be published, however, at the close of the investigation which is still in progress.

weight and then ascertaining the mean annual production for each percentage-group. To make the matter more clear both methods will now be applied—first to a differentiation of the flock on the basis of production groups.

For the purposes of the present inquiry the flock[8] may be divided into two groups on the basis of the mean annual production which was 120 eggs. One group was made up of individuals whose production was above the mean, and the other group included birds whose production was below. The mean production of the plus group was found to be 143 eggs, while the mean production of the minus group was 99. After these production-groups

TABLE I

SHOWING THE PRODUCTION OF THE FIRST LAYING YEAR OF GROUPS OF BIRDS SELECTED FOR DIFFERENT PERCENTAGES OF INCREASE OR DECREASE IN MEAN EGG WEIGHT, MEASURED AT THE PERIOD OF THE VERNAL (APRIL) WEIGHT MAXIMUM

PERCENTAGE-CLASS: Birds Selected for Increase in Egg Weight as Indicated Below	Number of Individuals Making the Record	Mean Production for the First Laying Year	PERCENTAGE-CLASS: Birds Selected for Increase in Egg Weight as Indicated Below	Number of Individuals Making the Record	Mean Production for the First Laying Year
> 10 per cent.	2	147	> 4 per cent.	17	132
> 9 " "	3	142	> 3 " "	21	122
> 8 " "	4	141	> 0 " "	31	122
> 7 " "	6	140	< 0 " "	6	111
> 6 " "	9	137	< 6 " "	28	112
> 5 " "	16	134			
			Total flock...	37[9]	120

had been established the mean net increase or decrease of egg-weight for each group was computed. The mean increase for the plus group was 5.4 per cent., and for the minus group 2.6 per cent. These results appear to indicate that, on the average, birds which manifest a greater percentage of increase in the weight of April eggs are likely to be the better producers of the flock.

In utilizing the second method mentioned above, the

[8] The flock in question consisted of 38 white Plymouth Rocks hatched in April, 1909. Some of the birds have now completed their seventh laying year.

[9] One hen, showing no increase and no decrease in mean egg-weight, and a production of 91 eggs, was omitted from the records.

birds of the original flock were divided into groups according to the percentage of net increase (or decrease) in the mean weight of eggs laid during the period of the vernal maximum (April). The percentage-groups were based on the scale indicated in the accompanying table.

From the data presented in Table I it is apparent that, on the average, the birds that showed the higher percentages of increase in the weight of the April eggs were also characterized by the higher productions. Those characterized by a weight-increase of 10 per cent. or more showed a mean production of 147 eggs, while those characterized by a weight-increase of more than 3 per cent. only, showed a mean production of only 122 eggs. The mean production of the group characterized by a *decrease* in egg-weight was the lowest of all—111 eggs, this being below the mean production of the entire flock.

If the birds are divided into two groups only, one having an increase of 6 per cent. or more, the other showing an increase of less than 6 per cent. or an actual decrease in egg-weight, it is found that the high-percentage group gives a mean production of 137 eggs, while the low-percentage group gives a production of only 112. In this instance the portion of the flock falling in the high-percentage class was approximately 24 per cent.; and this small group gave an average production that was 23 per cent. in excess of the production of the low-percentage group. The fact is thus brought out that, although a certain small proportion of high-producing individuals that are also characterized as manifesting only a slight percentage of increase in egg-weight at the period of the vernal weight maximum, will usually be found, the higher producers are, on the average, characterized by the larger percentages of increase (6 per cent. or above); and the selection of hens on this basis results in the separation of those individuals possessing the highest producing value.

V

In view of this correlation between numerical production and percentage of increase in egg-weight when measured at the period of the vernal maximum, it appeared desirable to ascertain whether a similar correlation existed between production and increase in egg-weight manifested at the *autumnal* (September) maximum. The same two demonstrational methods used in the previous instance may be applied.

The data on production were first re-distributed in such a manner as to group the percentages of increase or of decrease in egg-weight under two headings: (1) hens having an individual annual production greater than the mean (120 eggs), and (2) hens having an individual annual proportion of less than the mean production of the entire flock. In this way it was brought out that the plus group, with a mean production of 151 eggs, showed a mean net increase in egg-weight for September of 5.8 per cent., while the minus group with a mean production of 105 eggs showed a mean net increase of only one per cent.

TABLE II

SHOWING THE PRODUCTION OF THE FIRST LAYING YEAR OR GROUPS OF BIRDS SELECTED FOR DIFFERENT PERCENTAGES OF INCREASE OR DECREASE IN MEAN EGG-WEIGHT, MEASURED AT THE PERIOD OF THE AUTUMNAL (SEPTEMBER) WEIGHT MAXIMUM.

PERCENTAGE-CLASS Birds Selected for Increase in Weight Indicated Below	Number of Individuals Making the Record	Mean Production for the First Laying Year	PERCENTAGE-CLASS Birds Selected for Increase in Weight Indicated Below	Number of Individuals Making the Record	Mean Production for the First Laying Year
> 10 per cent.	8	143	> 4 per cent.	—	—
> 9 " "	9	141	> 3 " "	18	131
> 8 " "	10	140	> 0 " "	25	127
> 7 " "	11	142	< 0 " "	8	108
> 6 " "	12	141	< 6 " "	21	111
> 5 " "	14	137			
			Total flock...	33[10]	120

When the second method was applied, and the data redistributed so as to give the percentage-classes, the results shown in Table II were obtained.

[10] Four hens included in Table I were not employed in the present computations, because they failed to lay during September.

From the data presented in Table II it appears that, on the average, the birds that manifested the higher percentages of increase in the weight of the September eggs were characterized by higher annual production. Those showing a weight-increase of 10 per cent. or more gave a mean annual production of 143 eggs, while those birds characterized by a decrease in mean egg-weight showed an annual production of only 108 eggs. When the flock was divided into two groups according as the egg-weight had increased by more than 6 per cent. or less, the high-percentage group gave a production of 141 eggs as opposed to 111 eggs laid by the low-percentage group. Thus, dividing the flock on the basis of a 6 per cent. increase in the mean weight of all the September eggs, gave a group of 12 hens (out of 33) which showed a mean production 17.5 per cent. higher than the flock average (120), about 27 per cent. higher than the mean production of the low-percentage group, and 30 per cent. higher than the mean production of the small group of eight hens which manifested a decrease in mean egg-weight at the period considered.

It will hardly be necessary to call the attention of the reader to the circumstance that this method of demonstrating the correlation involved in the frequency distribution of these two variables (increase in egg-weight and numerical production) is, by its very nature, such as to constitute a practical application of the means involved.

The correlations between weight-increase and production, considered in the foregoing paragraphs, were so obvious that the question arose as to whether satisfactory correlations could not be demonstrated between these two variables under conditions in which a smaller amount of statistical data was employed. For instance, if the method should prove of value to poultrymen in affording a means for the detection of the higher producers of the flock, it would be desirable to reduce the machinery of computation to the lowest point consistent with valid results. It thus appeared pertinent to inquire whether computations based upon the weight of only ten eggs, laid as closely as possible to the periods of the absolute vernal

and autumnal maxima, respectively, would afford a satisfactory basis for establishing the weight-production correlations.

To this end, therefore, the mean weight of ten eggs laid by each member of the flock between the eleventh and twenty-fifth days of April[11] was computed, and the difference between the mean weight of these ten eggs and the mean weight of the first ten eggs laid by that hen at the beginning of her laying performance calculated as a percentage-increase or as a percentage-decrease. It should be added that the production during April was conducted at so rapid a rate that, in the case of 28 individuals out of 37, it was possible to obtain the record of ten eggs within the dates mentioned. In the remainder of individuals it was necessary to transcend these limits slightly. In no instance, however, was it necessary to take eggs from a date earlier than April 8, nor later than April 29. The data thus acquired were redistributed according to the percentage groups, and the results summarized in Table III.

TABLE III

SHOWING THE MEAN ANNUAL PRODUCTION FOR THE FIRST LAYING YEAR OF GROUPS OF INDIVIDUALS SELECTED FOR VARYING PERCENTAGES OF INCREASE OR DECREASE IN EGG-WEIGHT, COMPUTED ON THE BASIS OF THE WEIGHT OF TEN EGGS LAID AT THE PERIOD OF THE VERNAL WEIGHT MAXIMUM

PERCENTAGE-CLASS: Birds Selected for Increase in Weight Indicated Below	Number of Individuals Making the Record	Mean Production for the First Laying Year	PERCENTAGE-CLASS: Birds Selected for Increase in Weight Indicated Below	Number of Individuals Making the Record	Mean Production for the First Laying Year
> 10 per cent.	3	142	> 2 per cent.	27	123
> 8 " "	7	141	> 1 " "	29	122
> 7 " "	9	140	> 0 " "	31	120
> 6 " "	13	138	< 0 " "	16	106
> 5 " "	19	127	< 6 " "	24	114
> 4 " "	22	126			
> 3 " "	23	125	Total flock...	37	120

From the data presented in Table III it is clear that the small group of hens characterized by a percentage-increase on egg-weight of more than 10 gave a higher mean production (142 eggs) than any group manifesting a smaller percentage of increase in egg-weight. Each suc-

[11] See footnote on page 383.

ceeding group, characterized on a smaller percentage-increase, gave a correspondingly smaller annual production, until, when we reach "< 0 per cent.," the group manifesting a decrease in mean egg-weight, we find a mean annual production of only 106 eggs. When the flock is divided according as the mean percentage of increase is more than 6, or less than 6, we find that in the high-percentage group there are 13 hens with a mean annual production of 138 eggs, while in the low-percentage group there are 24 hens with a mean production of 114 eggs. In other words, upon the division point of 6 per cent. increase, one may separate about one third of the flock whose annual production is 15 per cent. higher than the flock average and 21 per cent. higher than the mean production of the remainder of the birds.

If the reader will now make a comparison of the results reported in Tables I and III, it will be seen that the correlation demonstrated through the employment of the "ten-egg method" is as clearly established, and as valuable from the practical point of view, as the correlation demonstrated through the use of a full month's production.

VII

In view, therefore, of these results obtained from the weighing of ten eggs at the period of the vernal weight maximum, it seemed desirable to ascertain whether the same "ten-egg method" at the period of the autumnal weight maximum would also serve to distinguish a group of hens characterized by the possession of higher producing ability. Accordingly the production data for September were analyzed from this point of view.

In explanation of the September results, however, several points should be noted. In the first place, although September production represents a definite mode in the annual production curve when plotted on monthly ordinates, in the case of the flock studied the month's production falls considerably short of the April production. In April all members of the flock, without an exception, were laying. In September there were four hens that did not lay at all; and three hens laid only three eggs or less.

In the redistribution of the data for the present purpose the records of no hens are included that did not lay at least five eggs in September. Two hens laid nine eggs five eggs. So that, in reality, the results of this case are based upon the mean weight of somewhat less than ten eggs from each hen.

In the second place it should be noted that the September production was scattered when compared with the April production; and although an attempt was made to secure eggs laid during the latter half of the month, it frequently happened that it was necessary to include eggs laid in the earlier part. The results of this redistribution of data and the attendant computations are presented in Table V.

TABLE IV

SHOWING THE MEAN ANNUAL PRODUCTION FOR THE FIRST LAYING YEAR OF GROUPS OF INDIVIDUALS SELECTED FOR VARYING PERCENTAGES OF INCREASE OR DECREASE IN MEAN EGG-WEIGHT, COMPUTED ON THE BASIS OF THE WEIGHT OF TEN EGGS OR LESS, LAID AT THE PERIOD OF THE AUTUMNAL WEIGHT MAXIMUM.

PERCENTAGE-CLASS: Birds Selected for Increase in Weight Indicated Below	Number of Individuals Making the Record	Mean Production for the First Laying Year	PERCENTAGE-CLASS: Birds Selected for Increase in Weight Indicated Below	Number of Individuals Making the Record	Mean Production for the First Laying Year
> 13 per cent.	3	147	> 2 per cent.	21	125
> 11 " "	6	145	> 1 " "	23	125
> 10 " "	7	143	> 0 " "	26	124
> 9 " "	8	144	< 0 " "	5	108
> 8 " "	10	144	< 6 " "	19	112
> 7 " "	12	139			
> 6 " "	12	139	September production 3 eggs or less..	7	96
> 5 " "	14	135	Ditto, plus hens showing decrease in wgt.	12	101
> 4 " "	16	134	Total flock.....	31	120
> 3 " "	19	131			

From the data presented in Table IV it appears, as in the former case, that higher production is correlated with the higher percentages of increase in egg-weight. The maximum group-production (147) occurred in those hens whose mean increase in weight was above 13 per cent. Selecting above 10 per cent. gave seven birds whose mean production was 143 eggs. Selecting above 6 per cent. gave twelve hens whose mean production was 139 eggs. On the other hand, selecting *below* 0 per cent. (*i. e.*, birds

showing a decrease in egg-weight) gave five hens with a mean production of only 108. When we add to these the hens that laid three eggs or less in September, we obtain a group whose mean production was only 96; and when we consider the hens that (1) gave a September production of 3 eggs or less, and (2) gave a decrease in egg weight, we obtain a combined group of 12 whose mean annual production was only 101 eggs for the first laying year.

A comparison of Tables I, II, III and IV thus shows that the last case presents the clearest evidence yet obtained for the positive correlation existing between percentage of increase in egg-weight and total annual production. The results are more definite than those obtained for the "ten-egg test" at the vernal weight maximum, or for the "month test" at either the vernal or the autumnal weight maxima. In other words a test based upon a smaller number of eggs, laid nearer to the absolute mode, gives a clearer indication of innate producing ability than does a test based upon a larger number of eggs laid in a "scatter grouping" about the approximate mode. This conclusion is in harmony with the views expressed by Gavin[12] and by Wilson[13] to the effect that the best unit of time for measuring a cow's milk-producing ability is not the year test, nor the thirty-day test, nor even the seven-day test, but the one-day test conducted when the production reaches its maximum. Apparently the measurement of egg-production in the domestic fowl, considered as a quantitative performance, rests upon a similar basis.

VIII

In bringing this paper to a close the writer wishes to have it distinctly understood that nowhere in these pages has it been stated that there exists in the domestic fowl a

[12] *Jour. Royal Agricultural Society*, 1913, 73. *Jour. Agricultural Society*, 1913, 5, 309–319. *Ibid.*, 1913, 5, 377–390 (on authority of Pearl).

[13] *Proc. Royal Dublin Society*, 1911, 13, 89–113. Jour. Dept. Agriculture, Ireland, 1913, 13, (4) (on authority of Pearl).

correlation between egg-production and egg-weight. Most poultrymen believe that, if a hen produces smaller eggs, she consequently produces more eggs; and, conversely, that if a hen produces larger eggs, she produces fewer eggs. This matter has not been considered in the present paper; it will be dealt with at a later time. The point may again be stated, that the significant correlation exists between numerical production and *the ability on the part of the hen to manifest an increase in egg-weight* at those seasons of the laying year when both production and egg-weight attain their respective maxima. A higher percentage of increase and absolute mean egg weight for the entire year has not yet been attempted. Many points like this remain to be worked out and the author does not wish to present his results dogmatically, but only with the hope that the problem will be attacked by other investigators. It is not improbable that the results may be found to vary with the breed of fowl, the date of hatching, the housing, the feeding and with other factors.

With these points openly in mind, and only with the purpose of stimulating further investigation and discussion, the author presents the following brief summary as expressing a biological fact which, if later proved to be of general application, may take its place as a fundamental law of production in the domestic fowl:

The innate egg-producing ability of a hen is manifested, not only by the number of eggs laid within a year, or within some shorter or longer period of time, but also by the degree of increase or decrease in the mean weight of her eggs, when this increase or decrease (calculated as a percentage-increase or as a percentage-decrease) is measured at those periods of laying (the vernal and autumnal maxima) characterized by the markedly increased production of the flock; and on this basis, groups of hens characterized by higher producing ability can be differentiated as accurately as, and more easily than by any other known means.

SOME HABITAT RESPONSES OF THE LARGE WATER-STRIDER, GERRIS REMIGIS SAY

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I. Introduction

The large water-strider, *Gerris remigis* Say (Fig. 1), is one of our most interesting and familiar aquatic bugs. During the years 1911–1913 inclusive, I made a somewhat intensive study of the responses of this species to the physical conditions of its environment, in the vicinity of Urbana, Illinois. This study has been continued, intermittently, up to the present time. The present publication forms only a part of the entire investigations. Part of this paper treats of observations made near Urbana, Illinois, and part treats of observations made near Syracuse, New York.

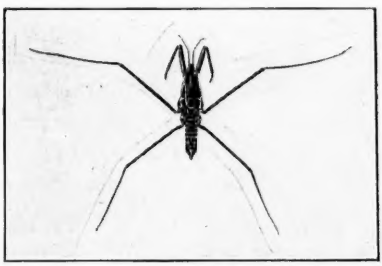

Fig. 1. The large water-strider, *Gerris remigis* Say, natural size. (Folsom.)

In the summer of 1911, a severe drought occurred in the vicinity of Urbana. In fact so extended was the period of dry weather, that many streams in the region, that usually were to be classed as permanent, became absolutely dry, and others were reduced to a few isolated pools. Water-striders, *Gerris remigis,* were trapped in many of these pools. As the gerrids belonging to this species are *mainly* apterous insects, they were unable to migrate by flight to some other body of water. Because of this, serious results might accrue to those individuals that were isolated in such situations when the pools became dry. It was a matter of interest to know what would become of these hemipterons when the water entirely disappeared. Therefore, a number of observations were recorded in connection with this subject.

II. PHYSICAL CONDITIONS OF AND BEHAVIOR IN BROOK HABITAT DURING SEVERE DROUGHT AT WHITE HEATH

1. Description of Physical Conditions.—Frequently certain physical conditions were found to exist in brook habitats in the early stages of droughts that had a direct bearing upon the very existence of the water-striders, *Gerris remigis,* but which will be mentioned here only very briefly. I wish to refer particularly to a brook, flowing partly through a forested region, near White Heath. This brook is situated about eighteen miles southwest of Urbana, and the physical conditions to be considered are such as existed during a drought, in the summer of 1911. During the earlier periods of this drought, I often found, in the drying brook bed, small pools of water not entirely isolated from each other. (In Figs. 2 and 3 are shown drought stages in the bed of the brook near White Heath that are very similar to those under consideration. The only difference here, of importance, is that the pools are in somewhat earlier drought stages than are those to which I have referred.) Such pools were connected by means of riffles not more than 6–12 inches wide (Figs. 2, 3). When food became scarce in pools of this character, or when a scum, often due to bacterial growth (Fig. 3), formed on their surfaces, I have observed that the water-striders made their way from one pool to another, by means of these small riffles of water, until such connecting links disappeared and the majority of the pools became dry. Eventually the gerrids were concentrated on the surfaces of the few isolated pools that remained. Sometimes, the bacterial growth, which was of a gray color, caused the death of hundreds of water-striders.

Such pools as have been mentioned persisted longer in that part of the brook's course that extended through the wooded region. In such a region the pools were larger, with a greater volume of water, than frequently was the case in more exposed situations. These conditions were due primarily to the protection afforded to the

FIG. 2. Detail of brook near White Heath, along margin of forested region, showing conditions during early stage of drought (July). Arrow indicates direction of current. *a*, pools on surface of which water-striders, *Gerris remigis*, live; *b*, small riffle connecting two pools; water-striders pass from one pool to another by means of such riffles. *c*, dry bed of brook exposed during early and late stages of drought. (Original.)

water by the surrounding trees. There, undoubtedly, is less evaporation in a region of this character than is true in those parts of the brook which are situated in regions lacking both trees and shade and are thus exposed to a high temperature and to the full effects of the summer sun. Very similar facts were observed near Charleston, Illinois, by Adams (1915, pp. 65, 66) in connection with a small stream in a forested area. He makes the following statement:

> This small temporary stream in a ravine formed the southern boundary of the area examined. . . . At the season of our examination it was a series of small disconnected pools. . . . On the surface of the pools were numerous specimens of a water-strider, *Gerris remigis*. The forest cover is undoubtedly an important factor in the preservation of such pools, as it controls the evaporating power of the air.

In this connection I wish to state that the water-striders were found in far greater numbers, in the late spring, summer, and early fall, on those portions of the stream that flowed through the forested area. After several years of observation, I have come to the conclusion that the shade and lower temperature are the important factors in influencing the gerrids to remain in such situations. De la Torre Bueno (1911, p. 246) has observed somewhat similar facts, as is indicated in the quotation:

> It [*Gerris remigis*] is to be found most frequently on running waters, although it also frequents still, but to a less extent, . . . They congregate in groups in shady, slow-moving parts of streams, at the tree roots projecting from banks into the water, in the shadow of bridges, and in general in almost any place where they have some shelter from the burning rays of the summer sun.

This observer (1917, p. 201), again writing of *Gerris remigis*, states that:

> These beasties are common and familiar sights to the lover of the quiet flowing waters running to the distant seas. In these haunts, in some still little bay or moveless backwater, under a bridge, or in the shadow of a tree, or in the cool recesses of an overhanging bank, you may see *remigis* gathered in numbers, rowing silently about. . . . Here they rear large families and spend at ease the sultry dog-days.

FIG. 3. Detail of brook near White Heath, along margin of forested region, showing conditions during a later stage of drought than indicated in Fig. 2 (August). Arrow indicates direction of current. *a*, pool showing scum on surface, formed by bacterial growth, which frequently kills water-striders in large numbers; *b*, pool on surface of which water-striders, *Gerris remigis*, live, surface being free from scum; *c*, small riffle connecting two pools; water-striders pass from one pool to another by means of such riffles. *d*, dry bed of brook exposed during early and late stages of drought. (Original.)

Attention has been directed to some of the physical conditions that existed in a brook water-strider habitat in the early stages of a drought. Certain features of such a habitat in the later drought stages, will now be considered. So far as I am aware, there are no records in the literature on aquatic Hemiptera, that describe the responses of these gerrids, *Gerris remigis,* after the water in their habitat actually disappears. The statements given here are very condensed records taken, chiefly, from my extensive field notes of the summer of 1911.

Late in the summer a trip was made to White Heath for the purpose of examining a number of isolated pools, on the surface of which water-striders were trapped, in the bed of the brook near that place. Particular attention was directed to one pool in which the water had evaporated rapidly during the previous week. It was examined about 8:00 A.M. and at that time the dimensions of the pool were approximately $12 \times 5 \times \frac{1}{2}$ inches. It was evident that the pool would be completely dry before night. Evaporation was taking place rapidly, as the atmosphere had been very dry for several days. The heat had been intense for a number of weeks, a temperature of 90°–100° F. not being uncommon.

2. Behavior in a Drying Pool.—There were twenty gerrids trapped on the surface of the pool, already mentioned. The insects did not move about very much on the water-film. They were very quiet, frequently remaining, practically, motionless for several consecutive minutes. There was no behavior on the part of the hemipterons that indicated any attempt to escape from the unfavorable surroundings. The members of this species being *largely* apterous forms and their optimum habitat being permanent brooks and streams of moderate size, with a current of medium velocity, it was, perhaps, not strange that they evinced no responses that showed definitely adapted behavior of a character suitable to cope with such abnormal conditions and unusual habitat changes. At 2:00 P.M. the pool was almost dry, and by 3:30 P.M.

there was little to indicate its location except a small area of rapidly drying mud.

3. Hibernation and Æstivation.—A number of writers —among others Uhler (1888, pp. 268, 275), McCook (1907, p. 265), and Kellogg (1908, p. 198)—on water-striders have stated that during hibernation these gerrids burrowed into the mud, under the banks of streams, at the bottom of water under stones and roots of trees, and at the bottom of the pool under roots or stones; and as some investigators, Tower (1906, p. 245), for example, consider hibernation and æstivation to be "fundamentally one and the same process," I was interested to observe whether the gerrids would burrow into the mud, or into some moist, sheltered spot under the banks of the brook, and remain there in a quiescent condition, a state of æstivation, until the rains came. However, there were no definite responses of such a character. I doubt whether water-striders do æstivate in the true sense of that term during periods of drought, although more evidence is necessary before a definite statement can be made.

As will be noticed later, the gerrids do respond to contact stimuli. Generally such a stimulus resulted in inhibiting locomotor activity and the insects remained motionless with their bodies closely applied to some solid object in the bed of the brook. This might prove to be a piece of dry mud, a stick of driftwool, a stone, or a clump of dead leaves. As a result of contact stimuli, sometimes they would crawl under such objects as have been enumerated and remain there for a considerable length of time. I have recorded observations which prove that they stayed in such situations from a few seconds to thirty minutes. The gerrids might remain in places of this character for even a longer time, as I have noticed, on a few occasions, that they were still there when I discontinued my observations for the day. However, I was not able to find them in the same situation the next day, even after most carefully marking the place. On several other occasions, I have made similar observations, but never

was I able to discover the water-striders the following day.

It is possible that there was a trifle of moisture in such situations, and that it was this, acting as a stimulus, which kept the gerrids there. But during these periods of drought—accompanied, as they are, in the prairie regions of Illinois with excessive temperature and glaring sunlight—I have failed, positively, to find moisture in such places, except under large objects, which were not present in the bed of the brook. Frequently, however, I have noticed that under these objects, in the brook bed, the temperature was slightly lower and of course there also was the shade. It is probable, also, that evaporation is reduced in such protected places. That certain arthropods are sensitive to the evaporating power of air is known from the work of Shelford (1913, pp. 85–102), who has demonstrated that the yellow-margined millipede, *Fontaria corrugate* Wood, and the ground beetles, *Pterostichus adoxus* and *Pterostichus pennsylvanicus,* respond negatively to the increased evaporating power of air. Therefore, lower temperature, shaded surroundings and reduced evaporation may be the three factors that accounted for the water-striders remaining in situations of the character that have been mentioned. Of course if it was proved that they stayed there day after day, this would indicate the possibility of their tiding over a short drought in such protected places. I have noticed on a few occasions, at least, that the gerrids have died, if away from water, when exposed to glaring sunshine and high temperature, during a drought. I also have observed, when the water in my laboratory aquaria was allowed to evaporate gradually, until it entirely disappeared, that, sometimes in a few hours and at other times in a few days, the gerrids died. This occurred, not infrequently, when the temperature was only 85° F.

During hibernation, there is no question about water-striders remaining quietly in one place for a long period of time. But it must be recalled that the temperature is

low at such times, and that they frequently seek dry situations. The subject of hibernation will be discussed at greater length in another paper. I may say, however, that these water-striders do not hibernate in any of the situations mentioned by the writers quoted, except under the banks of streams, and then away from the water. The positive thigmotactic responses of these gerrids have been observed frequently during periods of hibernation. At such times they formed tangled masses, which were due to the water-striders crowding closely together. These facts were recorded in my field notes as early as the winter of 1912–1913. It is interesting to recall that Essenberg (1915, pp. 397, 400) has observed similar responses in the case of *Gerris orba* Stål.

4. Initial Responses in Dry Bed of Brook.—With reference to the water-striders in the dry bed of the brook at White Heath, it appeared as if the gerrids might die right where they were, for no movements occurred for approximately ten minutes after the water had disappeared. Soon they began to move away. I can not state absolutely what was the stimulus that caused the initial locomotor responses, although it probably was the total disappearance of all moisture, as this was the only evident change in the external conditions. Singly and in small groups they jumped and walked in an ungainly fashion from the site of the former pool. While it was evident that the water-striders were less accustomed to locomotion on the land than they were to locomotion on the water, yet they made fairly good progress along the bed of the stream.

The evidence that the initial locomotor responses of the gerrids, away from the pool in which they had been living, were due to the drying up of the water is further supported by the following facts: Very frequently, I have observed that when water-striders were removed from the surface of a pool in a stream, or from an aquarium, where they had been kept in captivity, and placed on the ground or on some other solid surface that they at once

became active. This was true even if the gerrids previously had been inactive. They would start to jump and walk in a very agile, if ungainly, fashion, and made rapid progress, especially if they were on a smooth and level surface. It seemed that the transference from the water-film to a solid surface—for all other conditions were as before—was a sufficient stimulus to cause internal changes in the insects, that set free energy in the form of locomotor activity. To induce this result there was but one change in the conditions. In this connection I wish to direct attention to a statement by Jennings (1906, p. 285):

> Often, of course, stimulation does rouse an organism to increased activity. But even in this case the activity is due to the release of internal energy. It may, therefore, continue long after the stimulation which inaugurated the release has ceased to act. Such continuance thus does not necessarily imply continued action of the stimulus. In many cases the specific stimulus to action is only the *change* of conditions.

At this point, it may not be out of place to refer to certain observations which, possibly, may have a bearing on the locomotor responses of the gerrids, after the drying up of the pool of water. Abbott (1918, p. 234), in connection with some responses of land isopods, *Oniscus asellus* Linn., *Porcellio rathkei* Brandt, and *Porcellio scaber* Latreille, to humidity and evaporation makes the following statement:

> So far as observation shows, the effect on land isopods of exposure to a dry atmosphere, including the first effect in desiccation experiments, is an increase of activity. This is a useful adaptation, provided the activity carries them to other regions where moisture conditions approach more nearly the optimum.

5. Behavior in Dry Bed of Brook.—About ten yards farther downstream there was another much larger pool. Its dimensions were approximately 3 yds. × 2 yds. × 5 in. I was interested to observe how many, indeed if any, of the gerrids would reach it, and thus tide over the period of drought. Of the twenty water-striders present, eight of them went in the direction of the large pool, and wandered in an aimless, awkward manner down the dry

channel of the brook. They first tried one path and then another. These trials resulted in bringing some of the gerrids to the pool, but with reference to other water-striders the trials were not so successful. I found that it was a rather strenuous task to watch all the gerrids, although some of them frequently would come to rest. Without giving too many details, it may be stated that all of the eight individuals, already mentioned, made their way in a blundering fashion to the large pool of water. The first one reached the pool in 5 minutes and 30 seconds, and at once moved on to the surface-film. The time required by the others to reach the water varied to a considerable extent, the average being 14 minutes and 30 seconds. The last individual, of the group of eight, to reach the pool was fifteen minutes in making the journey. Three gerrids out of the eight traversed the distance to the pool with only a limited number of stops and deviations from the straight path. However, this is not to say that the journey was free from erratic movements, on the part of these insects, for such was not the case. The stops usually occurred when the gerrids came in contact with some obstacle in their path. It was not at all uncommon, when the insects moved away from their stopping place, for the direction of locomotion to be changed. The five other water-striders frequently came to rest against stones and pieces of dry mud. Such contact pauses were evidences of the thigmotactic propensities of the gerrids. They also wandered from side to side of the stream bed, trying one direction for a certain length of time, and then, through the influence of some stimulus, trying another. However, they made only a very few turns, directly away from the downstream pool during their entire journey. One water-strider crawled under a piece of dried mud and remained there for three minutes.

There were twelve other water-striders that have not yet been accounted for. Four of these wandered upstream and from one bank of the brook to the other, first

in one direction and then in another. Sometimes they stopped with their bodies in contact with some obstacle in their path. Frequently the direction was changed when they renewed the journey. The gerrids did not turn downstream for any considerable distance during any of their movements. After having observed them at intervals for two hours, I decided that they were unlikely to reach the water unless they eventually should wander to a pool somewhere upstream. The gerrids had moved away from the site of the former pool to such a distance that the water-strider farthest upstream was twenty yards distant from the starting point. One individual crawled under a piece of driftwood and had been in that situation for twenty-five minutes, when the observations were discontinued. Another water-strider crawled among some dead leaves and was there for twenty minutes previous to the time when I left that place for observations elsewhere.

Of the eight gerrids that have not yet been mentioned, three of them stumbled toward the left bank and five of them wandered toward the right bank of the brook. There were various obstacles in their paths, such as small rocks, pieces of dry mud, dead leaves and driftwood. The water-striders frequently came to rest in close contact with these objects—response to contact being a prominent feature in the behavior of these gerrids—and at length two of those, that had moved toward the left bank of the stream, jumped into a large crack formed in the baked mud of the bed of the brook. I observed these individuals from time to time—for at least three hours—until I left for Urbana, at about 8:00 P.M., and they were still in the same situation. I should state here that I searched for these two gerrids the next day and was unable to find them; nor was I able to discover the other four water-striders—to which I already have referred—that had moved upstream. The responses of one of the three gerrids, that in the first instance had moved toward the left bank of the brook, have not yet been described completely. After reaching the

bank, it began to jump downstream. This continued for a distance of two yards, when it turned, facing the right bank of the stream, and jumped in that direction for one yard until it reached the middle of the brook channel. The gerrid again turned to the left, this position directing its head downstream, in which general direction it continued to travel, until after a number of stops, it eventually reached the large pool. In passing over its entire route, this water-strider consumed fifteen minutes.

It has been stated that five gerrids wandered toward the right bank of the brook, on first leaving the site of the former pool. Three of these turned toward the middle of the stream channel and jumped upstream for two yards, turning to the left, they moved toward the bank of the brook. On reaching it, first one and then the others jumped downstream. After a number of pauses and deviations to the right and left, two of them again turned with their heads directed upstream and continued to jump and walk in that general direction. After several erratic movements and two stops, the third gerrid turned upstream. The insect jumped in this direction for three yards and then stopped with its body in contact with a small rock. It remained in that position for five minutes. In the meantime, the two other water-striders had worked still farther upstream. On going back to observe the gerrid that had been resting against the rock, it was discovered that this insect had moved away and was walking upstream. These three water-striders were observed for 1 hour and 50 minutes and as there appeared to be less and less possibility of their reaching the large pool downstream, the observations were discontinued. When the insects were noticed last, they had traveled eighteen yards upstream away from the site of the former pool, from which they originally came. One of them had crawled under a piece of driftwood and had been there for thirty minutes when I discontinued my observations. These gerrids were sought carefully the following day, but I was unable to find them.

The wanderings of two water-striders, out of the group of five, have not yet been traced. Attention already has been directed to the fact that, when they first left the site of the former pool, they wandered toward the right bank of the brook. After reaching this point, they turned downstream. I lost sight of one of them, while observing some of the other gerrids, and I was not able to find it again. The other water-strider proceeded downstream, but frequently deviated from a straight path, and often paused with its body in contact with various obstacles. After wandering downstream for three yards, it made a complete turn and its head thus pointed upstream. This occurred as the insect was making a jump, it appearing to lose control of the orientation of its body. The water-strider moved upstream for two feet and then turned to the left, walking in that direction for four feet. The gerrid made a wide turn to the left, so that its head was directed downstream. After much erratic wandering, it eventually blundered upon the large pool of water, having consumed forty minutes in completing the journey. It was a task of considerable difficulty to keep all the gerrids under observation, because, being almost the color of the background, they were very inconspicuous.

I have records of a number of other instances, treating of my observations on the responses of water-striders after the drying up of several other stream pools, at different times and during different seasons. Some of these pools were at distances of less than ten yards, while others were eleven, twelve, and fourteen yards distant from the original pool on which the water-striders lived. It is true that, on the particular occasion to which attention has been directed and which has been described in some detail, half of the gerrids entrapped on the surface of the pool escaped—when it became dry—to another and larger body of water by making their way overland. But in all the other instances that were recorded in my field notes of similar phenomena, a very much smaller percentage of the entrapped gerrids was successful in reaching

other bodies of water, after their own pool became dry. I will give a few examples: In one instance, only two of the trapped water-striders out of eight reached the surface of another pool, situated about eleven yards upstream. This was not accomplished until the gerrids had consumed much time in random movements, stops and deviations from the straight path. At another time, fifteen gerrids were trapped in a pool along the bed of a brook that had become almost dry, owing to a prolonged drought. Five water-striders out of the fifteen, or one third of the total number, only, were able to find another pool of water seven yards upstream from the point where they had been isolated. These gerrids made many trials, errors, delays, and turnings before they reached the surface-film. On still another occasion forty water-striders were entrapped on the surface of a stream pool. After the water had disappeared entirely, the hemipterons left their haunts, and stumbled along the dry bed of the stream. After much wandering about, first in one direction and then in another, six of the gerrids found a pool fourteen yards downstream from their former abode. The method of reaching the water seemed to be due to a crude form of trial and error.

I am inclined to believe that in periods of severe drought, large numbers of apterous individuals die. If the drought is a short one, and there is a certain minimum of dampness under logs, stones, and clumps of dead leaves, some of the water-striders may survive in such situations. In regard to physical conditions of a somewhat comparable character, Kirkaldy (1899, pp. 109, 110) makes the following statement with reference to *Gerris lacustris* Linné:

> In small wayside pools or streamlets which dry up periodically, it is obvious that the apterous inhabitants will die out unless their habitat is sufficiently near to a larger stream or pond; a severe drought occurring over a fairly large area, prolonged sufficiently to dry up all the water within that area, would cause all the species represented in that year by apterous individuals only to become extinct.

After having observed the erratic, aimless character of the responses of these insects, *Gerris remigis,* their ability to reach some other distant body of water appeared doubtful, and very largely dependent upon chance. If a larger body of water should be very near to the haunts of the gerrids, then there would be, through their random journeys, a greater possibility of reaching such a situation. In any case, their responses appear to be responses of repeated trial and failure, until through many repetitions, some gerrids eventually reach the water.

III. Experiments in Connection with Brook Habitat at White Heath

1. Methods.—I now wish to record certain other observations, on the responses of *Gerris remigis,* of a somewhat different character from those that already have been discussed. Many times when out in the field collecting *Gerris remigis,* some water-striders escaped while I was picking them out of the net as it lay on the ground near the margin of the stream. I noticed that these individuals generally found their way back on to the surface of the water. Several times it occurred to me to undertake a series of experiments with these water-striders for the purpose of discovering their ability to return to their own habitat after having been removed from it. The plan was to place them on the ground at various distances away from the stream and observe whether or not they returned to the water, and if so to record the readiness with which this was done. Up to the present, the experimental work has not yet been carried out as extensively as was desired, but the results that have been obtained may be of interest. The experiments were undertaken at the brook near White Heath, that has been mentioned previously. The site selected was at a place where the bank was flat, with its surface raised only a few inches above that of the water.

2. Responses When Facing Brook.—Twenty gerrids with their heads turned directly toward the stream, were

placed on the ground one yard away from the water. All those that got back to the water succeeded in reaching the surface-film in less than one minute. Similar experiments were repeated for four successive times, using different individuals. There was no experiment in which more than two gerrids failed to reach the water. Other experiments were carried out in which the water-striders were placed two, three, and four yards away from the stream. The majority of the gerrids found their way back to the water. All those that were taken two and three yards away from the stream were back on the surface-film within 2 minutes and 30 seconds. Gerrids that were placed on the ground four yards away from the brook evinced a few more random movements than was the case of those nearer to the water. A slightly smaller percentage of the gerrids found the way back to the stream from this distance. Those water-striders that reached the surface of the brook did so within four minutes from the time each experiment began.

3. Responses When Parallel with Brook.—A series of experiments were performed identical to those already described, except that the water-striders were placed on a line with the long axis of their bodies parallel with the bank of the stream. It was observed that the majority of the gerrids were successful in reaching the water. In the case of some individuals, a longer time elapsed than was true in the first series of experiments. But most of the water-striders either turned at once toward the brook, or else they did so a few seconds after the first locomotor movements began. In some instances the gerrids jumped, for one yard or more, in the direction in which their heads were directed, before turning toward the brook. Some gerrids were placed on the ground four yards distant from the water. Certain individuals of these evinced some hesitancy in jumping directly toward the brook and there were a number of random movements.

4. Responses When Facing Away from Brook.—Other experiments, of a character similar to those that already

have been described, were carried out, in which the heads of the water-striders were directed away from the stream. As before, a majority of the gerrids were successful in reaching the water. The same distances were used as in the former experiments, but in all cases there was less promptness in moving toward the brook. It also was evident that there were more random movements than was true in the former experiments. Of the water-striders placed four yards away from the stream, a greater percentage, than in any of the other experiments, failed to reach the water.

These hemipterons employed two methods of locomotion on land, walking and jumping. The more rapid progress was made by jumping. The individual jumps averaged about one inch in length, although frequently they were greater than this. Sometimes when jumping, the gerrids made a complete turn, as if they were not able, fully, to control the orientation of the body, and this frequently resulted in changing their direction of progress. In this connection, I wish to direct attention to the fact that Essenberg (1915, p. 399) has observed a somewhat similar response on the part of *Gerris orba* Stål and makes the following statement:

> It runs with a jerking motion, making from four to six jumps in succession and then making a short stop. Very often it turns a somersault and continues running without interrupting its course until it reaches a place of safety. There it lies quietly for from fifteen to twenty-five minutes, then suddenly begins its race again.

Often during one of these jumping movements, the water-striders struck against some obstacle and when this occurred, their direction of progression was changed. There was some tendency for the gerrids to continue to move in the direction which they already had taken, without regard to the position of the stream. In the three series of experiments it is of interest to notice that the water-striders reached the brook with a fair degree of directness. It was only in the series of experiments, in which the heads of the gerrids were turned away from

the water, that there was much evidence of lack of promptness in turning toward the brook and of random movements.

5. Experiments with Barrier.—In the experiments that have been described, I felt that the sense of sight was the important factor in directing the water-striders to the brook. However, there was always the possibility that moisture, as a stimulus, might influence the responses of the gerrids. Therefore, I planned a number of experiments for the purpose of obtaining more definite information on this subject. My idea was to arrange a sort of barrier, along a limited section of the bank of the brook, which was to be of such construction as to permit diffusing moisture to pass through it readily, but on the other hand, the barrier was to be so opaque that the reflection from the water of either sunlight or diffuse daylight could not be seen through it. The barrier first employed was made of roughly interwoven leafy branches of trees. This was held in position by wooden stakes. It was four feet high and extended along the bank of the stream for a distance of approximately fifteen yards. There were two reasons for employing a barrier of this character: first, the readiness with which the materials could be obtained; and second, the fact that its appearance was in harmony with the general environment. However, it was found to be practically impossible to make it sufficiently tight so as to prevent reflections from the surface of the water from passing through it.

A number of series of experiments were tried with water-striders placed on the ground one yard and also three yards away from the brook. While the experiments were not sufficiently satisfactory, as a basis for definite conclusions, it may be stated that the gerrids displayed somewhat less promptness in reaching the water than was the case when such a barrier was absent. However, in general it was evident that the water-striders jumped toward the brook with considerable expedition and definiteness. If the barrier had proved to be light-

tight, I should have been inclined to explain such a measure of promptness in moving to the water as due, perhaps, to the effect of moisture from the stream.

Of course if the means of finding the brook was mainly through vision, it was to be expected that the gerrids would be a little less prompt in reaching the water, because less reflection from the surface of the brook could be seen than was true with the barrier removed. Unfortunately the movements of the leaves by the wind permitted many bright reflections to pass through the barrier. Lying flat on the ground, with my eyes as close to it as practicable, in order to take a position as nearly the same as possible to that of the water-striders, I found that I was still able to see the bright sunlight of the hot summer day, reflected from the surface of the water. I planned to use another form of barrier and test this matter fully, when, because of the character of the season, I was compelled to take up certain other observations, and I have not yet had the opportunity to supplement this work with additional experiments.

(To be continued)

ONCHIDIUM AND THE QUESTION OF ADAPTIVE COLORATION[1]

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I. As the result of his extensive study of the phenomena of coloration in the tropical reef-fishes, Longley (1917) comes to the conclusion that the colors of these animals are "correlated with their habits in such a way that their conspicuousness is thereby reduced. There is no correlation of bright colors with special modes of defence, and no evidence that bright-colored types enjoy immunity greater than that of their fellows." The general tendency of this, the most recent, and in many ways the most thorough, observational study of the question of adaptation in animal coloration that has yet appeared, favors adherence to the view that as a whole the coloration of animals is of a concealing, hence adaptively protective kind. There are instances among invertebrates which seem fundamentally inconsistent with this interpretation, because the organisms in question are not colored like their habitual background and because they are for this, and for other independent reasons not inconspicuous. It is also a fact that in specific instances of this sort special repugnatorial systems are frequently encountered. Because of its importance for the theory of adaptation, this matter is deserving of careful scrutiny. The natural history of the littoral pulmonate *Onchidium floridanum* Dall affords a case in point.

[1] Contributions from the Bermuda Biological Station for Research and from the Anatomical Laboratory of the Northwestern University Medical School.

In his notes on the fauna of Bermuda, Heilprin (1889, p. 187) described as new the form "Onchidium (Onchidiella) trans-Atlanticum," saying of its pigmentation: "smoke color or dark olive," in dorsal view. Some years later Pilsbry (1900, p. 503), reporting on Verrill's collections at Bermuda, listed this species under the designation by which it is now known, *Onchidium floridanum* Dall, adding, "Its dark olive green color agrees so well with the stains on the rocks that it is very inconspicuous. —(A.E.V.)"

It is possible, though not certain, from these brief descriptions, that Heilprin and Verrill had respectively collected representatives of the two differently colored types of *Onchidium* which occur at Bermuda. No constant morphological differences have been detected upon which to base a separation of these types, one of which is in nature blue-black in general aspect, the other characteristically of a light ("smoky") olivaceous hue. Some intermediate shades do occur, but in the field a rather sharp separation of the two groups is always possible. The very dark form is much the more numerous. So far as pigmentation is concerned, it is probable that the blue-black types contain merely a heavier deposit of a subepidermal pigment, which is chemically alike in both kinds of Onchidia. Nevertheless, we are inclined to believe that the difference in pigmentation may be representative of a real genetic difference between the two types, for reasons which may briefly be stated as follows: The two modes of colorization are in many places exemplified side by side; young and adults of maximal size are found belonging to either group; the occurrence of the pale form is independent of the seasons, and is found year after year in the same spots; in other places, where blue-black individuals may be very numerous, no lightly pigmented individuals have ever been encountered. Therefore we are probably dealing, not with a temporary fluctuation in color, determined by some metabolic cycle, but with constitutionally diverse races of *Onchidium;*

whether they be "species" or not is immaterial. A similar phenomenon seems to occur in other genera of this family, for v. Wissel (1898) notes that Plate (cf. Plate, 1893, 1894) collected two differently colored forms of *Onchidiella coquimbensis* Plate; in this instance the color difference between the two types, found in nature under the same stones, was not so pronounced as we find it in *O. floridanum*. In an *Onchidiella* which is very abundant at Bermuda one of us has observed a corresponding sort of color difference. The variation in question cannot be in any way a sexual one, for reasons already given and because the animals are hermaphroditic.

II. *Onchidium floridanum* lives between tide levels. It is never seen when the intertidal shore-zone is covered by the sea; during this period it inhabits deep crevices and cavities within the eroded rock. A dozen or more Onchidia live together in this way. Their "nest" opens to the exterior by means of a small opening, usually further obstructed by the growth of *Modiolus,* which is almost invariably quite inconspicuous, although the densely packed small black "mussels" within and around the entrance itself may form a very definite patch standing out clearly amid the olive-brown algæ covering the surface of the rock. When the tide falls so far as to have left the level of the *Onchidium* nest about an hour before, the occupants of that nest emerge, successively and in a steady stream, wander some little distance over the rock, feed, and then, before the tide has risen again, the members of each group synchronously begin to creep back to the respective nests from which they originated. The remarkable character of this "homing" behavior we have separately studied with considerable care, and have discussed in another place (Arey and Crozier, 1918). Here we would emphasize simply the fact that *O. floridanum* appears in the open, on the intertidal rocks, only during the period of low water.[2]

[2] The experimental observations from which some of these statements are derived will be found described in a report, by Arey and Crozier, on the general behavior of *Onchidium,* now in course of preparation.

The nature of the background provided on this rock surface is by no means constant in different localities. Commonly, as in the case of rocks in sheltered places, completely submerged at high water, or on lee-shores generally, the limestone is covered by a feltwork of matted Enteromorpha and associated plants, sun-bleached to an olivaceous yellow tint. This is the usual condition within the sounds (Great Sound, Castle Harbor), and along the shores of bays protected from the surf. In places not so sheltered the rock may be colored by minute green algæ, or by the growth of calcareous green or reddish algæ, with which, as in the former case, many serpulas are intermingled; barnacles (*Tetraclita porosa*) also sometimes extend into the *Onchidium* zone, but the prevailing hue, and the color of any particular spot of small area, is largely determined by algæ.

Onchidium also occurs, though more sparingly, on surfaces of a still different type. In some places the layer of red earth (clay) underlying superficial æolian limestone is exposed at the water's edge, usually in a small cove containing loose rocks left bare by the tide. This earth provides a muddy layer of reddish brown, over which Onchidia are occasionally found to be creeping. From such tiny coves the direct rays of the sun are largely excluded, so that the in-shore surfaces of the stones are overgrown by algæ, green or red-brown, unbleached; on these surfaces also *Onchidium* may be found.

The zone inhabited by *Onchidium* is also that in which great numbers of the common *Modiolus* occur. These mussels, as already stated, almost invariably line the approaches to the *Onchidium* nest, and grow within the entering passageway itself. Except where the mussels have settled thickly in more or less horizontal fissures between the wind-formed strata of the limestone, they occur conspicuously in little groups, frequently not more than 3–4 cm. in diameter, occupying every slight depression in the surface; this is usually the case in situations

where *Onchidium* abounds. A typical instance is that of a low-lying islet such as "Little Agar's," in Great Sound. The leeward (eastern) side of this island, well protected by the mass of the larger Agar's Island, exposes at low water a considerable extent of almost horizontal rock-surfaces, whereon occur sparingly clumps of *Modiolus,* which are, however, much more abundant around the vertical edges of these slabs. Within the clumps of *Modiolus* lie many *Onchidium* nests, from which the mollusks at appropriate times creep out and wander over the flat, yellowish, sunlit beach-rock. It is in situations of this general type that *Onchidium* is most abundant; in less freely illuminated places it occurs more sparingly.

It is a remarkable fact that, in addition to frequenting characteristically, though not exclusively, habitats which during most of the day receive the direct rays of the sun, *Onchidium* comes out into the open only during daylight hours, and never (in our experience) at night;[3] the circumstance which makes this condition particularly curious is that *O. floridanum is at all times negatively phototropic,* and in a very precise manner, when tested apart from its usual environment. No more striking instance is known to us of apparent contradiction between the findings of analytical experimentation regarding the behavior of an animal and the actual life of the same creature, for the snails when on the intertidal rocks move toward or away from the horizontal rays of the sun with perfect indifference. The cloudiness or brightness of the day, however, has but an unimportant influence, if any, on the numbers of Onchidia which appear upon the rocks.

[3] That the wandering of *Onchidium* into the open is determined by immediate physical circumstances in the environment is shown by two facts: No rhythmic periods of migration persist when the snails are removed to the laboratory; on days when strong winds and accompanying ocean currents prevent the escape of the tidal water from the partially enclosed Great Sound, so that there is no occurrence of "low water," the Onchidia remain within their nests. *O. floridanum* can remain below water for 6–7 days without much impairment; a small amount of air is sometimes trapped within the rock cavities which they utilize as nests and this may in some cases help during prolonged submersion.

III. The coloration of an *Onchidium* is the result of its intensity of pigmentation, the texture of its mantle, and the activity of its mantle glands. An adult measures, on the average, 17 mm. long by 12 mm. broad by 6 mm. high when resting undisturbed, being then oval in outline; but it becomes more elongated during creeping, measuring then about 24 by 8 mm., and also less strongly arched, being about 4 mm. high. When first observed creeping out of its nest for an "airing," it usually happens that the *Onchidium* bears upon its dorsal surface a thin pellicle of slime completely investing the mantle. This slime-layer is tough, somewhat thickened about the periphery, and can be peeled off intact. Frequently it is so removed, mechanically, during the snail's efforts to creep out of its nest, for the passage-way is commonly so obstructed by the growth of *Modiolus* that the Onchidia must slowly insinuate themselves through slit-like apertures not more than 2 mm. wide. Occasionally Onchidia are seen creeping about with their slime-coat but partially removed.

The slime-pellicle is encrusted with the muddy fecal matter which the Onchidia deposit. This calcareous mud is swallowed when feeding on the algæ over which they creep in the open; it represents the ordinary silt of the shore-line, and may in part be chemically abstracted from the thin layer of sea-water covering the algæ as the tide recedes, owing to photosynthetic removal of CO_2, especially as accelerated by increasing temperature and by isolation as the sea-weeds become more exposed.

When the slime-coat is removed, an ordinary *O. floridanum* seems at first sight jet black; the mantle is wet and shiny; closer examination shows that the color is really a very dark and smoky olive. The mantle-glands continue their slime secretion, however, and by the action of wind and sun the surface of the mollusk quickly assumes a dry blue-black aspect, its numerous papillæ glistening in the light; the appearance of the mantle, with a slight "bloom" on it, then resembles that of the tiny

thysanurans so common on the beach sands and also on the rocks where *Onchidium* lives.

Typical Onchidia of the lighter colored variety appear at all times light olive or greenish yellow. The coloration is not "solid," for the dark-hued internal organs usually show through as one or more dull blackish patches. The edge of the mantle is quite pale.

It has not been possible to convert one variety into the other by laboratory experimentation. The dark Onchidia are still dark after six weeks complete starvation.

Onchidia of either type are found, frequently side by side, in all the varieties of habitat which we have described. To mention some specific localities, we may list the following: Bailey's Bay; Flatt's Inlet; the east side of the channel entrance to Hungry Bay, at its inner end; an island at the entrance to Fairyland Creek; a small cove on Burgess Point. These situations are widely separated, and cover the different sorts of backgrounds to which allusion has been made.

Not only in a general way, merely, but also in any given place where they occur, the lightly pigmented Onchidia fail to indicate by any peculiarity of behavior that they differ essentially from the dark ones. That they are easily overlooked is quite true; they are homochromically colored in practically all the instances that have been observed. But it should be noted that in one such place the homochromically colored individuals were found in the proportion of but one to nearly 190 of the dark black ones. This was on a greenish rock surface of northern exposure where a color-changing form like the isopod Ligia was pale-greenish, like the pale Onchidia. In other places more or less isolated colonies of fifty or more pale specimens were encountered. In some cases the coloration was decidedly greenish, rather than olivaceous yellow.

IV. The numerous dorsal papillæ of *Onchidium* were said by Semper (1877, 1881) to indicate the location of mantle-eyes; since, however, no sensory nerve-terminals

were at that time demonstrated within the "eyes," some writers followed Joyeux-Laffuie (1882) in the opinion that the "dorsal eyes" of Semper were in reality mantle glands, corresponding to those undoubted glands found in the related genus *Oncidiella* (cf. v. Wissel, 1898). The more complete analysis of the histology of the mantle in *Onchidium* by Stantschinsky (1908), nevertheless, proved that the "dorsal eyes" do possess nervous structures appropriate for photoreception, the retinulæ being, in addition, of the inverted ("vertebrate") type. *O. floridanum* possesses, however, repugnatorial mantle glands as well, which are not associated with the dorsal papillæ, these last being relatively minute.

The glands are comparatively large, but are easily overlooked in an undisturbed *Onchidium*. The periphery of the mantle has a frilled appearance, numerous tiny projecting papillæ giving it a serrated outline. If the animal be disturbed, the tissues surrounding each of the glands become erected, and 14 stout conical papillæ, 7 on either side and each with a terminal pore, become evident.

If the animal is suddenly removed from the rock by grasping its dorsum, the glands all become turgid, point upward and inward, and quickly discharge by the contraction of their muscular investments. The secretion is a viscid fluid, milk-white, non-miscible with sea-water, and decidedly acid in reaction. Discharge is more easily elicited in air than when the animal is submerged. Under water the secretion is expelled in long threads, but in air the stream as it leaves the gland breaks up into a fine spray, which may be shot to a distance of 12–15 cm. With vigorous stimulation, as when the back of the *Onchidium* is pinched with forceps, the discharge appears from every gland as a stream about 0.2 mm. in diameter, which is often too stout to form fine droplets. In nature, however, the initial repugnatorial discharge is less copious; it then takes the form of a fine spray, almost invisible, which may be thrown for as much as 15 cm., or about ten times the length of the *Onchidium*.

As discharged, the secretion is found to contain at least three elements visible under the microscope: a clear fluid, apparently water; clear droplets of oily appearance; and a finely granular material. The relative proportions of these three elements vary in different glands. An individual tested in the field, without previous disturbance, usually secretes a densely granular mass containing a fair proportion of globules; in the activation, a good deal of the secretion usually adheres to, or falls back upon, the mantle of the snail itself—the dryness of the mantle, and the fact that in nature violent discharges of secretion seem seldom to be invoked are important for the freedom of the mantle from being itself sprayed. On the other hand, the fluids ejected from one or more glands of starved Onchidia are almost always clear and watery, sometimes with no trace of the granular or droplet constituents. Variation in the composition of the corresponding glands of *Oncidiella* was recognized by v. Wissel (1898), who also discovered the secretion droplets (globules) which previous workers had overlooked.

When the secretion adheres to the mantle of an *Onchidium,* it can be seen that considerable stimulation of the skin is thus produced. The secretion is quite acid to litmus and to a number of other indicators. The acid is found in the dense granular material, and does not occur, save in relatively slight amount, when this material is absent. The white masses may be caught on a clean slide and studied under the microscope. The granular material is insoluble in 90 per cent. alcohol, or in water. If indicators are added to a drop of the secretion under a coverslip, it can be seen that the acid slowly diffuses out into the water from the granular mass.

The white substance, if received on the tongue, is found to sting with considerable persistence, like wild mustard. It does not taste sour. Plate, testing the secretion of the mantle-glands of an *Oncidiella,* found "dass das Secret dieser Drüsen auf der Zunge ein deutlich wahrnehmbares

Brennen hervorruft" (according to v. Wissel, 1898, p. 597).

Stimuli initiated in several different ways may serve to elicit discharge of the repugnatorial glands, as: stroking the mantle with a blunt point; pinching it with forceps; applying solutions (5/8 M) of NaCl or (less effectively) LiCl, or of methyl or ethyl alcohol (5 M). Touching or pinching the *periphery* of the mantle itself does not lead to discharge, but on the contrary leads to localized retractive puckering; when a gland is erected, preparatory to discharge, stimulation of that gland causes it to collapse, or "wilt," a process similar to that following normal discharge. If the dorsum be scratched until erection of the glands occurs, and then NaCl solution be applied to one of them, it is found, as also with mild faradization, that two or even three successive discharges are possible; but the later out-pourings of secretion are very meager as compared with the first explosive ejection. About twenty-four hours is required for the regeneration of a new supply of repugnatorial substance.

Before the glands discharge, the edge of the mantle curls upward, forming around the *Onchidium* an encircling saucer-edge bearing the now prominent glands; thus, depending upon the intensity of excitation, the glands come to point more and more dorsal, and in this position release their contents. Not only so, but the individual gland papillæ are themselves further directed in a general way toward the irritated spot, so that if a slip of glass be held horizontally 1 or 2 cm. above an *Onchidium* the back of which is at one point subjected to mild faradic excitation, the contents of all the discharging glands are found to impinge upon very nearly the same point; sometimes all the discharges converge to an area of less than 3×3 mm., immediately above the spot irritated. The gland or glands nearest the site of stimulation are the first discharged, and the implication of more distant ones depends on the degree of stimulation.

Semper (1881, p. 372 *et seq.*) considered that the dorsal

eyes of *Onchidium* were employed in sensing the approach of animals which might attack it. He had in mind the attacks of the Philippine *Periopthalmus,* a blenny which, leaping along the intertidal beach-zone on its ventral fins, feeds upon *Onchidium* and on arthropods. Semper thought that the eyes of the Onchidia rendered them sensitive to the shadow of an approaching fish, whereupon a fluid spray was discharged from the skin glands, driving off the fish. Whether Semper regarded the secretion as repugnant to the fish in a chemical way, or whether it was to act in a mechanical fashion merely, is not altogether clear. Semper endeavored to make out a correlation, illustrative of evolutionary principles, between the geographical distribution, on the one hand, of the species of Onchidia possessing dorsal eyes, and, on the other, of the genus *Periopthalmus.*

There are at Bermuda no *Periopthalmus,* nor any other fishes of similar habits. *Onchidium floridanum* is, however, quite reactive to shading. The dorsal surface of the mantle is the part sensitive to decrease of light intensity. In a moving *Onchidium* the ensuing reaction involves a momentary cessation of creeping, retraction of the tentacles, and a depression of the periphery of the mantle to the surface of the rock. The tentacles themselves, which in air are much further protruded than when under water, are not photosensitive. An *Onchidium* shaded when not creeping, but at rest or feeding, exhibits the same type of response, but the mantle is more decidedly humped. But, in our experience at least, no discharge of the repugnatorial glands is induced by shading. This does not mean that Semper's observations were not correct. In *O. floridanum* the type of excitation which is preeminently and characteristically effective for gland discharge is of a mechanical nature, namely, a rubbing or pinching of the apex of the dorsum or a dislodging thrust upon the ventral surface of the girdle. Although when tested by faradic excitation it is found that gland discharge *may* be elicited when even the sole of the foot is

irritated, or almost any other region (with difficulty in the case of the tentacles), a greater intensity of stimulus is necessary than when applied on the back of the animal. Mechanical stimulation is in general more effective than chemical. Of the chemical excitants tried, only NaCl, LiCl, methyl and ethyl alcohols were successful, whereas such substances as pure amyl alcohol, numerous acids, alkalies and salts, in concentrations corresponding to those used in the case of the successful stimulating agents, as well as a series of alkaloids, were without effect when small volumes (a half or one c.c.) were applied to the mantle.

The designation of the mantle glands as repugnatorial is justified by observations in nature. Isopods (*Ligia*) and grapsoid crabs (*Sesarma*, and in some places, *Pachygrapsus*) abound in certain spots where the Onchidia take their airing. By prolonged watching, unequivocal instances have been noted in which these arthropods have come into direct contact with Onchidia, and in every case the glandular discharge took place, followed immediately by the retreat of the crab or isopod; it is not implied that the crabs were endeavoring to eat the snails, but it seemed rather that it was important for *Onchidium* to avoid being accidentally pushed off the rock into the water; it does not adhere to the rock with any great firmness, particularly under water, is easily dislodged, and if purposely pushed over the edge of the stone on which it may be creeping into the water, the animal finds it difficult if not impossible to return to its own nest.

Tests upon a number of animals (*e. g., Fundulus* from the land-locked brackish ponds at Bermuda) which could not by any possibility have ever come into contact with the *Onchidium* secretion, have invariably demonstrated the powerfully repugnant character of gland contents.

Finally, it is important to note that *Onchidia of whatever degree of pigmentation possess these repugnatorial glands, and in an equivalent state of development.* The behavior of the differently pigmented individuals, with

reference to the use of the glands, is identical throughout the whole series.

V. Without entering upon a lengthy setting-forth of the several alternative possibilities which might readily be suggested in partial explanation of the foregoing series of facts, we may state our belief that an attempt at such explanation in terms of color-adaptation is confronted by serious, and indeed fatal, obstacles. From such a standpoint we are required to consider only the period during which the *Onchidium* is emerging from its nest and creeping about in the open. In the case of a dark *Onchidium* it is quite true that the animal is often very easily overlooked while in process of creeping over the *Modiolus* at the entrance of its nest; the color-match of the snail and the mussel is a fairly exact one. But an *Onchidium* does not resemble *Modiolus,* although of about the same size, when creeping upon algæ at the rate of 5 cm. per minute or faster. The paler kinds of *Onchidium* are, in almost every instance noted, a very precise match for their background, and sometimes careful search is necessary in order to detect them.

The non-homochromic pigmentation of the dark Onchidia would, in view of the presence and behavior of the poison glands, be of special interest in connection with the idea of warning coloration. But this view is rendered unintelligible in the face of the paler, concealingly-colored types. Moreover, the Bermudian Oncidiellas to which we have already alluded are very inconspicuous indeed, and they also possess poison-glands; their mode of life is quite different from that of *Onchidium,* since, although likewise inhabiting the intertidal zone, they retreat during low water to tiny holes,—dead serpula tubes and the like,—coming forth again only when covered by the water. These Oncidiellas are small, not more than 3 mm. long, and both light and dark individuals are in the highest degree difficult to detect when on their natural background; a delicate mottling of brownish pigment, in various shades, has a pronounced concealing effect in these

places. Perhaps this accounts for the fact that *Oncidiella* has not previously been reported from the Bermuda area.

Not only does the idea of "warning" coloration become untenable for *Onchidium,* but the idea of adaptive concealing pigmentation is likewise without adequate support. The dark Onchidia are not concealingly colored, except during the transitory interval of their actual emergence from the nest, and even there they are easily seen, if looked for. The slime-coating, which would undoubtedly be of assistance in making them look like the background (for the silt it contains is formed on rocks where the pale bleached yellow algæ grow, and in other places where *Onchidium* lives), is almost invariably removed before the snail begins its promenade in the open. The relatively meager proportion of the Onchidia found pigmented in a truly homochromic manner is not explained by the idea of adaptive coloration, although the variation in the hues of differently tinted individuals would seem to provide favorable ground for the operation of selection. In this connection it might be suggested that *Onchidium floridanum* represents a comparatively recent addition to the fauna of Bermuda, and that the dark pigmentation common to this species and its immediate relatives throughout the world is even now in process of modification; but the fact that in the neighboring genus *Oncidiella* two more or less distinct general modes may be of widespread occurrence forbids the placing of any special emphasis upon this possibility, and even then, if correct, it would have no final significance for the conception of selective color-modification. If nutritive conditions, and temperature or light, or all three, do operate in a manner favorable to Onchidia of one or the other type in different places, the habit of living together in colonies might lead to a measurable degree of inbreeding, tending to prevent the general distribution of one variety of pigmentation, but at the same time a paler coloration, probably recessive in genetic behavior, would to that extent achieve a greater opportunity for perpetuation.

Summary

VI. *Onchidium floridanum* Dall exhibits at Bermuda two chief types of pigmentation, a pale type tending to dull olive yellow, which tends to be concealingly colored, and a much more abundant type of dark blue-black appearance.

No correlation can be established between the pigmentation of an *Onchidium*—which there is some reason to consider the result of genetic factors primarily—and the hue of the substratum over which the snail creeps in the open at low tide.

O. floridanum possesses repugnatorial mantle-glands of an effective type, secreting a granular emulsion of substances having a strongly acid reaction and producing on moist surfaces of the human mouth a pronounced stinging sensation. Touch and pressure stimuli on the dorsal surface of the mantle are characteristically involved in releasing the discharge of these glands, which shoot their contents, in the form of a fine spray, to a distance about ten times the length of the *Onchidium* and with conspicuous accuracy of direction toward the source of excitation.

These facts are incompatible with the view that the coloration of *Onchidium* is determined or controlled by selection in the direction of homochromicity or concealment. Nor can they be understood in terms of "warning" coloration.

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THE CONTRIBUTION OF CARL FRIEDRICH VON GÄRTNER TO THE HISTORY OF PLANT HYBRIDIZATION

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IN the beginning of the nineteenth century, the question of the sexuality of plants was still undergoing a certain amount of disputation, despite Koelreuter's investigations; the history of which episode is sufficiently traceable through the writings of Schelver and Henschel. In order to assist in the settlement of the matter, the Royal Prussian Academy of the Sciences at Berlin, made public announcement in 1819 of an offer of a prize for the solution of the question—"Does hybrid fertilization exist in the plant kingdom." No response having been evoked, the Academy extended the competition period, and doubled the amount of the prize offered. On July 3, 1826, the prize was awarded, although not in its entirety, since the Academy did not consider the solution adequate, to Dr. A. F. Wiegmann, of Braunschweig.

In January, 1830, the question was propounded anew by the Dutch Academy of the Sciences at Haarlem, in the following language:

> What does experience teach regarding the production of new species and varieties, through the artificial fertilization of flowers of the one with the pollen of the others, and what economic and ornamental plants can be produced and multiplied in this way.

Since, by the termination of the contest period, January 1, 1834, no response had been received, the period was extended to January 1, 1836. In October, 1835, Carl Friedrich von Gärtner of Calw, son of a distinguished botanist, Joseph Gärtner, formerly professor at the universities of Tübingen and St. Petersburg, and who for a

considerable period of years had been conducting experiments of his own in hybridization, became aware of the offer of the Dutch Academy. Thus far, only brief reports of his work had appeared at Tübingen and Paris.

On account of the shortness of the time available, Gärtner sent to the academy merely a preliminary report of his experiments, accompanied by one hundred and fifty mounted specimen sheets of his different plant hybrids, which elicited a favorable response, and induced the academy to grant an extension of the time for sending in the completed work to December 30, 1836. The interesting resolution of the committee runs in part as follows:

> That, in view of the number of new results, which could only have been obtained through very manifold investigations over many years, the service of the author be acknowledged.

The requirements of the committee having been compiled with, the award was formally conferred on May 20, 1837. The thesis appeared in Dutch translation, as a document of 202 pages, in the *Proceedings* of the Academy for 1838. In 1849, a revised and greatly enlarged edition, "the fruit of unbroken, zealous, almost twenty-five years' work," was published in German at Stuttgart.

The writer has found nowhere in current literature any adequate presentation of this seldom read, and little referred-to work, and yet it contains not only much interesting information of a concrete nature, but a great deal of speculative philosophical insight in dealing with problems in hybridization, that shows a scientific mind of distinct value.

Sachs says of the writings of Gärtner (3), "The two works together, are the most thorough and complete account of experimental investigation into sexual relations in plants which had yet been written. They are a brilliant termination of the period of doubt with respect to sexuality in plants, which succeeded to the age of Koelreuter." "And thus it was," says Sachs further,—"in two small cities of Württemberg, that the foundations of the sexual theory were laid, and the theory itself perfected, so far as it could be, by experiment only, by three of the most eminent of observers, Camerarius of Tübingen, Koelreuter and K. F.

Gärtner of Calw, who contributed so largely to the empirical establishment of the theory, that all that was done by others would seem of small importance" (p. 427).

As Focke says ((1) p. 437), "In numbers of experiments, he has probably been surpassed by no other hybridizer."

An idea of the sheer laborious work which Gärtner's operations involved, may be obtained from the mere statement that he performed close to ten thousand separate experiments in crossing, involving nearly seven hundred different species belonging to eighty different genera, and from which some two hundred and fifty hybrid plants were produced.

From such a large mass of detail as Gärtner's memoir involves, it is difficult to derive a concise series of statements of the experimental results. In endeavoring to group the phenomena of hybridization upon a scientific basis, Gärtner undertook to classify hybrids into three divisions, according to their external habit: (1) "intermediate types," (2) "commingled types," and (3) "decided types," although, as he says (p. 277):

> There exists no exact delimitation among them, but they go variously over into one another, so that it is not seldom very doubtful to which of these forms, this or that hybrid should, with the greater right be assigned.

In regard to the intermediate types, Gärtner follows Koelreuter's view

> that, as in the fertilization of pure species, so also with hybrid breeding in the case of simple hybrids, a complete balance occurred of both fertilizing materials, either in respect to mass or activity. In this assumption he was still further strengthened through the similarity of types from reciprocal crossing. He believed further, that in the later generations of simple hybrids, and in the further grades of hybridization, where no such regular process of hybridization occurs, the inclination of types either toward the father or the mother, proceeded from the not quite complete balance, or the slight overbalance of the one or the other fertilization materials (p. 277).

Regarding the so-called "commingled" types, Gärtner says as follows (p. 282):

The second kind of types, frequently occurring among hybrids, is that with commingled parental characters, insofar as now this, now that part of the hybrid, approaches more to the maternal or to the paternal form, whereby, however, the characters of the parents in their transference to the new organism, never go over pure, but in which the parental characters always suffer a certain modification.

Under the third class of hybrid types, Gärtner considers those—

Among which the resemblance of a hybrid to one of its parents, either to the father or to the mother, is so marked and preponderating that the agreement with the one or with the other is unquestioned, and strikes one at once (p. 285).

Regarding the behavior of plant hybrids in the first hybrid or F_1 generation, as compared with their behavior in the second generation, Gärtner's remarks are interesting, although not based on numerical data.

Among plants, by far the greater number of the normal and regular types of the hybrids from the first cross, as compared with the exceptions and varieties, testifies against the operation of . . . external influences, and proves rather the inner necessity of a regular formative development according to law on the part of pure species in general as well as of hybrids (p. 275).

Regarding this supposed formative force (Bildungskraft), Gärtner arrived at a conception of the inner nature of the phenomenon occurring in hybridization, which is scientifically interesting, although amounting to a theory, and not to a conclusion directly derived from experiment.

The foundation and the determination of the types of hybrids, might therefore be discoverable, not so much in the mass and the relationship of the germinal materials, as in a vital modification of the formative force of the one or the other fertilization material (p. 270).

As we now know from Mendel's experiments, however, it is this modification of the "sexual material" itself which pre-determines the direction in which the formative force may run. In Gärtner's day, the abstract conception of a relative potency of the one or the other parent in hybridization, was a prevalent one, and this statement above is a philosophical conception, which

Mendel's data made it possible to precipitate into a definite morphological theory. Gärtner went so far, however, as to attempt to measure this "potency" in a definite manner, by means of the number of viable seeds produced in reciprocal crosses.

That this relation of the factors of the fertilization forces in the case of species of plants capable of hybridization, is definite and according to law, we assume from the fact that from such a hybrid combination come more or fewer good seeds to be sure, according to the favorableness or otherwise of the incidental circumstance, but that, however, in the case of every such hybrid combination, it is never able to produce over a certain maximum of viable seeds, which maximum is peculiar to each combination of that kind (p. 206).

Gärtner goes so far as to say:

The inequality in the strength of sexual affinity (Wahlverwandtschaft), in the case of the reciprocal combination of species, is a general phenomenon occurring in plants, and will therefore lead in time to the disclosure of the relations, and to a closer determination of the value in respect to magnitude of the individual factors of sexual affinity. This inequality establishes a scale of sexual affinity peculiar to each species, which lies in the difference in the relationship of the strength of the two factors. In this singular characteristic of plants, the peculiar nature of the species is most plainly recognized,—much more, in fact than in its external form (p. 200).

Those species which are able in crossing, to exert a preponderating influence upon other species, Gärtner calls "generic types."

Just as such generic types in hybrid breeding, are able, as it were, to gain the upper hand over the type of other species, so is the strength and form of these species broken and overmastered by others (p. 290).

This manifestation of generic types, according to which one species acts in a predominant manner over several other species in hybrid breeding, is a further uncontradictable proof, that the relationship of the forces, through which the union of two pure species takes place, must be unlike, and that there can be no question there of any balance of factors. To be sure in mixed hybrids, the relationship of the formative forces of the two sexual substrata appears to be tolerably alike; however, in them likewise, in this or that part of the hybrid, now the character of the one, now that of the other factor is more plainly expressed (p. 290).

Gärtner seems to see in this dominance of type in

species, a way of evolution which leads to the establishment of dominant family types.

The generic types appear to have their analogue in the natural family types, and, since the origin of family types has occurred according to certain laws, so will type-formation of hybrids, since it is not at random, but is constant, follow the same laws, according to which plant forms in general are formed and have developed (p. 291).

This brings Gärtner to a further statement regarding the possibility of recreating ancestral types, which is especially interesting as a pre-Darwinian view upon evolution. Referring to species of *Lobelia, Lychnis, Dianthus,* etc., he says:

If these nearly related species had once come from a common ancestral type, or had become separated from one another through the subsequent development of the one or the other individual, then it appears to be highly improbable that they would not again unite in their ancestral form through reciprocal crossing, or prove themselves to be analogous types in hybrid breeding (p. 163).

Gärtner then arrives at one of the most striking conclusions in the older literature on the then so much mooted species question, and which is the more interesting because it takes the physiological rather than the morphological point of view. He says:

The essential nature of a species, therefore, consists in the definite relation of its sexual forces to other species, which relationship, taken together with the specific form, is a characteristic, individual and constant one; for every species, form and essence in this regard are one (p. 163).

Gärtner frequently returns to a philosophical contemplation of the nature of the fertilization process in hybridization, characteristically as follows:

Not external resemblance in form and habit of species, but the harmony of the inner nature, gives the capacity for hybrid fertilization; both are not always harmoniously bound together (p. 186).

For an actual hybrid combination, a certain harmony in both sexual elements is, however, necessary, and precisely in this harmony lies the capacity for the union of two otherwise heterogeneous species (p. 110).

Only a harmony of the inner nature, on which the relationship of the germinal materials rests, which indeed is ordinarily accustomed to be associated with the external generic characters, but is not necessarily

bound up with concordance in external structure, determines the presence of the capacity for hybrid fertilization (p. 142).

In the following discussion, more or less in the same manner, Gärtner shows an intellectual freedom from the fetish of morphological species, and clearly demonstrates the possession of a physiological temper and attitude of mind.

Our investigation concerning harmony of forms in families and genera, have shown that we have to consider two different kinds of relationship among plants, an external and an internal: the former rests upon conformity in habit, *i. e.*, in growth, in the shape and form of the leaves, and in the harmony of the flowers and the organs of pollination: these, however, in their greater or lesser inclination to sexual combination of species in hybrid fertilization. The former might be identified with the morphological, the latter closely with the physical relationship. Now, since both are not infrequently met with in combination, for that reason, our predecessors have not clearly distinguished both kinds of relationship from one another, but have held both as identical, or rather, have regarded the inner relationship as an immediate consequence of the external, and assumed this as a law, so that the agreement of species in habit, not only favored, not merely the existence, but also the strength of the sexual attraction, and indeed likewise conditioned it (p. 166).

From the agreement of the external form and habit of species of plants it may not therefore be inferred, that the sexual powers and relationships must also agree therewith, as experience indeed teaches, that many plants, however congruent they otherwise are in form, and even in organs of fertilization, nevertheless possess little or no inclination to unite in hybrid fertilization. One of the best-known examples of this is *Pyrus* and *Malus*, which, despite their near relationship in habit and sex organs, from the testimony of other observers also, do not admit of fertilization on the one side or the other (p. 167).

In the following passage, the morphological and the physiological points of view are well contrasted:

The systematic genera are artificial syntheses, which are not united according to absolute laws, but according to arbitrary external characters, which indeed often harmonize with the inner nature of the species, but likewise not seldom differ from this, if indeed it is not to be questioned, that even these characters proceed from the inner organism, and are determined through it (p. 139).

Gärtner, no more than any other investigator in the field of hybridization of his day, with the possible excep-

tion of Sageret, had any conception of the idea of unit characters operating as such, and capable of being analyzed separately. The then prevalent idea was one of "potency" and "pre-potency," in the case of the hybrid types that Gärtner called "decided," *i. e.*, in which the dominance of the one or the other parent was plainly evident. A species in a cross was supposed to function as a whole as such. An idea of this older point of view is obtained from the following:

> Thus, just as there are species in a natural genus, which possess a prepotent fertilizing power upon several other species of their genus, so there are also species, which exert upon several others such a typical predominating effect, not to an equal extent, to be sure, but still of such a nature that their operation in all combinations is to be recognized by a character in common. Both of these forces, are, however of different kinds, and follow different laws (p. 289).

Gärtner did not regard Sageret's case of segregation of characters to be the normal result of hybrid fertilization. While it is true that Gärtner recognized in a certain sense the fact that parental characters often behave in a more or less unitary manner, he was led by the nature of his mind, as well as by the results of his observations, to take a synthetic rather than an analytical view of the hybrid organism.

> The explanation of the origin and development of the forms of the hybrids from the elements and characters of the parents, is as important for plant physiology as for systematic botany (p. 25), and further,—
>
> The laws of hybrid types orient themselves not toward the individual organs of plants,—do not apply to a single part, *e. g.*, stems, leaves, etc., but are applicable rather to the inner nature of species. The organs which determine the types of hybrids, must therefore be investigated and compared in their totality, and in their mutual interrelationship. For the most part, the individuality of a hybrid expresses itself in its entire habit, but in this respect, the flower above other parts of the plant, is most frequently and plainly distinguished (p. 251).

However, Gärtner's most fundamental view upon the question whether the plant as a whole, or its individual characters considered as such, determine the nature of the hybrid offspring, is expressed in the following clear manner.

In the formation of simple hybrids, as in sexual propagation in general, two factors are functional; this inequality of activity flowing out of the specific difference of species, expresses itself in the more marked or the more feeble emergence of the individual paternal characters in the different parts of the hybrid. Whether the species nature in its entirety and its formative impetus, determines the direction and form of the type, *or whether also the individual parts of plants have a special influence upon modifications, can only be determined through further investigations* (p. 257).

In the absence of what he deemed sufficient evidence to the contrary, Gärtner conservatively adhered to the view that the parent organism entered into the cross as a whole, rather than as a congeries of character-units, behaving in a manner separately, although often linked together.

Regarding the recognized instability of hybrids, Gärtner simply says, without distinguishing as to the generation:

Variability in the progeny of hybrids is a principal character of hybridity (p. 518).

So far as any distinction as between the first and second generation is concerned, Gärtner merely says:

The general laws of development of the parts of plants, hence appear to undergo through hybridization no change perceptible to the senses, but all the developments and changes of the hybrid plant body appear to follow the same laws as in pure species, the organs of reproduction, and the material ground materials of the cross alone excepted.

The latter behaves differently in the second generation and in the succeeding stages of hybrid fertilization, where, on account of the different nature of the two factors of the hybrid in the succeeding zygoses, an altered, shifting, variable direction in type-formation enters into the varieties thus originating (p. 572).

Concerning variability in hybrids of the second and succeeding generations, he says:

Other hybrids, and in fact the most of them which are fertile, present from the seeds of the second and further generations, different forms, *i. e.*, varieties, varying from the normal type, which in part are unlike the original hybrid mother, or deviate from the same, now more, now less (p. 422).

Perhaps the most definite allusion describing the con-

dition in general terms, of what we term segregation in the second generation, is the following:

Among many fertile hybrids, this change in the second and succeeding generations, affects not only the flowers, but also the entire habit, even to the exclusion of the flowers, whereby the majority of the individuals from a single cross ordinarily retain the form of the hybrid mother, a few others have become more like the original mother parent, and finally here and there an individual more nearly reverted to the original father (p. 422).

Regarding the matter of unusual vigor in hybrids, Gärtner remarks, giving examples, although again without referring to any particular generation—

The marked increase in the size of the flowers, is a phenomenon not seldom occurring among hybrids (p. 295), and—

One of the most marked and general characters of plant hybrids, is the luxuriance of all their parts, since among very many of them, an exuberance of growth and development of roots, branches, leaves and flowers manifests itself, which is not encountered among the parents, even under careful cultivation (p. 526).

Gärtner did not omit to apprehend the possible value of this fact to agriculture, although, of course, he did not recognize the first hybrid generation as a special phenomenon.

Among the characters of hybrids worthy of recommendation for agriculture, their tendency toward luxuriance in stalks and leaves, and their extraordinary capacity for tillering is related above. With respect to the raising of forage, agriculture could, without doubt, make great use of this characteristic (p. 634).

So far as genetics from the present technical standpoint is concerned, Gärtner's data of course are not of special interest, because his crossing was made upon species as units, and not upon the character-unit basis, and no records were made of the numbers of the different types secured from his crosses. It is of interest to note, however, that Gärtner's methods in his hybridization operations partially anticipated the rigorous methods of to-day, regarding the purity of parental types.

In order to judge with certainty concerning the nature of the types which have arisen, and in order to obtain entirely reliable results, it is

above all necessary that one be in advance in complete certainty concerning the species with which the experiments shall have been instituted, that they be specifically correctly determined, and that no doubt prevail concerning their purity (p. 252).

Finally, Gärtner's investigations upon color inheritance, which cover thirty pages of text, while not of genetic value from the modern standpoint, are interesting and valuable as a summary of the then existing knowledge on the subject. One observation upon intensification of color deserves mention:

Red with red, not seldom gives a heightened brilliancy of color, as is especially plainly shown in the flame-colored flowers of *Lobelia cardinalis, fulgens* and *splendens* (p. 315).

One of the matters of genetic interest is the fact that Gärtner experimented in the crossing of corn, with a view to determining the matter of change of color in the seeds due to crossing, as reported by Sageret. Unfortunately for Gärtner's experiment, however, he crossed a dwarf yellow corn without pericarp color, with corn having colored pericarp ("of red, gray and striped color"), instead of with colored endosperm. In consequence, of course, the seeds borne the first year, "differed neither in size nor in color in the least from the natural seeds of *Zea Mays nana* of the earlier sowings" (p. 322).

The following year, however, instead of getting complete color dominance, he obtained from one ear a ratio of 224 with non-colored pericarp, to 64 with pericarp colored. The other ear gave 104 seeds without pericarp color to 39 colored. He carried the seeds through to the next generation, but gives no numbers for them. Gärtner also crossed *Lychnis diurna* with reddish or dark-brown seeds, and *Lychnis vespertina* with ashy-gray seeds, finding no change occurring as the result of crossing, but obtaining what we should call dominance of ashy-gray in the first hybrid generation. From Gärtner's observations, therefore, he felt justified in stating as a law—

That the influence of the foreign pollen in hybrid fertilization, alters nothing in the forms and external characters of the fruits and seeds

peculiar to the mother plant, but produces in the embryo only, the capacity of bringing forth a mixed product from both concurrent factors, through the germination and the further development of the new plant (p. 327).

In an earlier paper of Gärtner's (2b), he cites Mauz's case of modifications in the character of different fruits on a pear tree, through pollination from various varieties of pears, whereby he was said to have obtained, "a great number of fruits different in form and colors" (p. 138).

His interest aroused by the phenomenon reported in maize, he undertook a series of crossing experiments to determine "whether foreign pollen exercises or does not exercise an immediate influence on the external character of the fruits and seeds which are the result of these fertilizations," but with entirely negative results. No change whatsoever was observed in the color or external characteristics of the fruits arising from crossing.

> The influence of the foreign pollen does not then change anything in the external forms peculiar to the mother plant, or in the external qualities of the fruits, the seeds and even the embryo. This influence only gives to the latter the faculty of producing, through germination and through the ulterior development of the new plant, an intimate combination of the form of the members of the two species which have united in its production (p. 139).

One of the most interesting matters, of course, is that which concerns the alteration in the character of hybrid seeds or fruits due to the immediate effect of foreign pollen. Gärtner reviews in detail the previous work of Knight, Goss and Seton with peas. In 1829, he started a selfed and a crossed series of peas, using four varieties (pp. 81–85).

1. Paris Wax (yellow seeds).
2. Dwarf Creeping (white flowers, yellow seeds).
3. Sugar peas (red flowers, wrinkled greenish-yellow seeds).
4. Early Green Brockel (white flowers, green seeds).

The results as to the immediate effect of the cross on the seeds were as follows:

Parents.	*Hybrid Seeds.*
Paris Wax (yellow) × Sugar Peas (greenish yellow)	yellow.
Paris Wax (yellow) × Early Green Brockel (green)	greenish-yellow.
Sugar Pea (greenish yellow) × Dwarf Creeping (yellow)	dirty-yellow.
Sugar Pea (greenish yellow) × Early Green Brockel (green)	no change.
Dwarf Creeping (yellow) × Early Green Brockel (green)	greenish-yellow.
Early Green Brockel (green) × Paris Wax (yellow)	dirty-yellow.
Early Green Brockel (green) × Sugar Pea (greenish yellow)	yellow.
Early Green Brockel (green) × Dwarf Creeping (yellow)	yellow.

As the above results show, the same dominance of yellow over green in the hybrid seed appears as in the experience of Knight, Goss and Seton.

Respecting identical results obtained as the result of reciprocal crosses, Gärtner makes the following unqualified statement:

> The most important and the most interesting phenomenon in the crossing of plants in hybrid breeding, is the complete similarity of the two products; since the seeds produced from the one as from the other fertilization, give rise to plants of the most complete similarity, so that their different origin and derivation, upon the most careful investigation of both kinds of hybrids, does not show the least difference in respect to their form and type; and even the most practiced specialist with a hybrid species, is not in position to distinguish the origin of the hybrid with respect to the sex of the parents (p. 223).

Gärtner's work is not only noteworthy for its remarkable extent with respect to the number of species experimented upon, but with regard to the care which he exercised in his operations, he says:

> For complete assurance of the purity and reliability of the products of hybrid breeding, and for testing the conclusions derived therefrom, we have repeated most of the experiments, especially the doubtful cases, not once only, but several times, and put them to the test through crossing of the same species, using different individuals of that species, for even with the most scrupulous foresight and precision, individual rare instances have still occurred in these tedious and wearisome investigations, where the suspicion had made itself felt, of a mistake or error having crept in, either in pollination or emasculation, since such results stood in direct contradiction to the usual experiences, and on a repetition of the experiments, made itself incontrovertibly evident as an error. We believed it possible to attain no higher degree of certainty in this branch of natural science, and to be able to bring the conclusions derived therefrom to no higher proof, than through the precise coincidence of

the forms of the products, by repetition under the same conditions with the same species, but with different individuals, and at different times (p. 675).

Again, in another place, he makes substantially the same statement with respect to testing what he refers to as "selective affinity."

In order to gradually get as close as possible to the true selective affinity relation among the species of plants, it is necessary not only that a greater number of experiments be instituted with the same species of plants, but also that the same experiments be repeated with different individuals and at different times, because as well in the female organs of a plant, as in the pollen of another species, a basis for different results may be concealed (p. 214).

Gärtner was not behind in realizing the practical utility of hybridization in agriculture and horticulture. The following somewhat extended extract shows his keen sense of interest to the possibilities latent therein, although his own scientific efforts did not lead him into economic experiments.

The heightened fruiting capacity of hybrids and variety crosses, deserves the most marked attention in respect to orchard, vineyard, and the whole of garden culture. The striking fertility of several orchard and vineyard varieties may find its explanation herein. It is, to be sure, to be surmised that this capacity does not reside in an equal degree in all variety crosses, and that this character would incline toward the peculiarity of the species; nevertheless it is to be expected with tolerable certainty, that with many valuable orchard and vineyard varieties, an increased yield might be able to be attained through crossing with other varieties. Improved sorts with weak or weakened vegetative power, united with other species of more vigorous growth, would promise an improved product with a longer life duration and a stronger structure of the plant body. As already many admirable stone, pome and vineyard varieties have been raised from seeds which had originated through chance crossing, so, through intentional artificial crossing of varieties, still many other sorts might be very easily produced. But to get definite results, and to be able to determine the outcome exactly, for the advantage of science as a whole, fertilization should not be committed to mere chance, but an exact and scrupulous procedure must be observed, with careful records of the varieties combined.

Of still more extended utility is hybrid breeding for æsthetic botany; for the latter, artificial fertilization opens a wide field for activity, enjoyment and achievement. For the fancier of ornamental plants, the

ease with which many hybrids are able to be produced, is an inexhaustible source of satisfaction and profit. He recognizes that he is in possession of materials with which he can busy himself, and he deliberates over the way and manner he can best and most profitably combine them; in that, he gives attention to the characters wherein each species characterizes itself, whether in the splendor of the colors of the flowers, the fineness of their delineation; fragrance, growth, form, quantity of flowers; whether endurance of the severity of our climate in this or that combination is to be taken into special consideration;—he will attempt to calculate provisorily with some probability the possible appearance of the hybrids, and he will finally be surprised at getting a plant which had never before existed in nature (pp. 638–639).

This concludes the matter of general interest in Gärtner's memoir. The writer believes that it should be carefully read by every plant breeder, not only for the details of practical and historical value therein contained, but because of the philosophical spirit underlying Gärtner's scientific attitude upon the nature of the hybrid organism.

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ON THE USE OF THE SUCKING-FISH FOR CATCHING FISH AND TURTLES: STUDIES IN ECHENEIS OR REMORA, II.

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II

In 1507, there was published at Venice by Franconzio another collection of travels, entitled "Paesi Nouamente Retrouati Et Nous Mondo da Alberico Vesputio Florentino Intitulato" [Countries Newly Found and the New World of Albericus Vesputius Called the Florentine]. Chapters LXXXIIII to CXIIII faithfully reproduce the Libretto of 1504, are in fact a second edition of the Libretto, and need not detain us.[14]

All this however simply pushes the question back one step further and it now becomes "What or who is the source of Peter Martyr's information?" The answer is that these sources are identical with those for the "Life of Christopher Columbus" by his son Ferdinand, for Las Casas's history of the West Indies, and for Bernaldez's "Reyes Catalicos." In addition Dr. Eastman has skilfully worked out certain internal evidence which points directly to the one person who gave to Peter Martyr the data incorporated in Chapter XV of the Libretto.

Let us first of all consider the account which Ferdinand gives of the fisherman-fish incident, which seems to

[14] The "Paesi" was also reprinted at Milan in 1508 by Arcangelo Madrignano, and later at both Basle and Paris by Simon Gryneo. Martyr says that this plagiarism was the work of Alvico de Cadamosto and denounces him in Decade II, book 7. Martyr's "First Decade" was published in 1511, and the first edition of the "Decades" appeared in 1516 under the editorship of the author's friend, Antonio de Nebrija. The publisher seems to have been Alcala de Henores.

have occurred on or about May 19, 1494. This is found in his "Historie" as follows:

> The nearer they sailed to Cuba, the higher and pleasanter the little islands appeared which were all over that sea, and it being a matter of difficulty and to no purpose to give every one of them a name, the Admiral called them all in general Jardin de la Reina, the Queen's Garden. . . . In these islands they saw crows and cranes like those of Spain, and sea-crows [gulls], and infinite numbers of little birds that sung sweetly, and the air was sweet as if they had been among roses, and the finest perfumes in the world; yet the danger was very great, there being such abundance of channels, that much time was spent in finding the way out.
>
> In one of these channels they spy'd a canoe of Indian fishermen, who very quietly, without the least concern, awaited the boat which was making towards them, and being come near, made a sign to them in it to attend till they had done fishing.
>
> Their manner of fishing was so strange and new to our men, that they were willing to comply with them. It was thus: they had ty'd some small fishes they call *Reverso* by the tail, which run themselves against other fish, and with a certain roughness they have from the head to the middle of the back they stick fast to the next fish they meet; and when the Indians perceive it, drawing their line they hand them both in together. And it was a tortoise our men saw so taken by those fishermen, that fish (the *Reverso*) clinging about the neck of it, where they generally fasten, being by that means safe from the other fish biting them; and we have seen them fasten upon vast sharks.
>
> When the Indians in the canoe had taken their tortoise, and two other fishes they had before, they presently came very friendly to the boat, to know what our men would have, and by their directions went along aboard the ships, where the Admiral treated them very courteously. . . .

The close similarity between the accounts of Ferdinand and Martyr will already have occurred to the reader. Before going further it is but fair to say that Ferdinand's original copy is not known, the printed text being from Ulloa's Italian translation. However, Ferdinand was the heir of the admiral, and, since all his father's papers which were preserved seem to have fallen into his hands, may be considered as his father's literary executor. Winsor says (pages 9–10) that "Ferdinand, or the writer of the 'Historie,' . . . it seems clear, had Columbus's journal before him." Columbus

kept a journal of his second voyage until he was stricken down by sickness, and this is attested to by both the Historie and by Las Casas.

Ferdinand himself on this point says (Churchill's Voyages, II, p. 560) that, after the fishing scene above described, the Admiral held on his course though worn out with fatigue, neither having had his clothes off nor lain in a bed since leaving Spain "till the 19th of May (1494) when he writ this," *i.e.*, the account of the fishing scene in Queen's Garden. Windsor states (page 39) that the "Historie" was up to 1871 believed to be a biography of Columbus by his son Ferdinand, and that though doubted by some, is still firmly held to by many authorities. With the above conclusions, Dr. Eastman and I, after a careful study of all the available data, found ourselves in full accord.

Las Casas, the great apostle to the Indians, left at his death a manuscript history of the West Indies. This had been long in the writing, from 1527 possibly, or more positively from 1552, to 1561 (Las Casas died 1566), but was longer in getting published (1875). However, in its manuscript form, it was available from the time of his death for all later historians.

Las Casas's account of the fishing scene need not detain us here since it is essentially like that in the "Historie" by Ferdinand, and like that in the "Libretto" of 1504 and the "Decades" of 1511. Much more important is the query as to the source of Las Casas's data. Winsor (pp. 39 and 47) quotes Harrisse that he thinks that both Ferdinand, or the author of the "Historie," and Las Casas had access to common documents or may be a manuscript prototype of their writings. And later (p. 56) Winsor speaks of "the journal of Columbus as preserved by Las Casas."

One further source of information needs to be set forth, and then after a brief consideration of Peter Martyr's sources, this part of our study will be finished. About the middle of April, 1915, Dr. Eastman got word

of a manuscript copy in the Harvard Library of a manuscript document in the Royal Library at Madrid of Columbus's time written by a personal friend of the great navigator and narrating the events of the second voyage. A few days later he wrote me as follows:

Everything run down thus far is overshadowed in importance by the new find, ante 1500, which I take to be the *ipsissima verba* of *Columbus* himself. The MS. . . . now in the Harvard College Library formerly belonged to Mr. Prescott, who had it transcribed from a MS. work in the Royal Library of Madrid. A part of it was translated, rather poorly, in the Massachusetts Historical Collection before 1850, and some years later the Madrid MS. was printed (1856 at Seville and 1870 at Madrid). Irving and Humboldt both consulted the original MS. or copies of it, and historians agree that the author, Bernaldez, an Archbishop of Andalusia, not only entertained Columbus at his house on his return from his second voyage, but received the journals and other papers then in Columbus's possession. Prescott makes this statement and it is repeated by others. Now Bernaldez, in his work written before 1500 embodies practically all of Dr. Chanca's[15] letter, and hence we may suppose that what he takes from Columbus's papers and journals was copied nearly verbatim. I regard this as one of the most important authentic sources for the second voyage . . . coming as it does nearest to the fountain head.

Let us now consider Bernaldez's account, which as just shown seems to be a transcription of Columbus's own words.

The Admiral set sail [from Jamaica] with his three caravels, and sailed 24 leagues towards the west, as far as the gulf Buen Tiemps. . . . On Whitsunday, 1494, they stopped at a place which was uninhabited—but not from the inclemency of the sky, or the barrenness of the soil,—in the midst of a large grove of pam-trees, which seemed to reach from the sea-shore to the very heavens. . . . Here they all rested themselves upon the grass about these fountains, enjoying the charming fragrance of the flowers, and the melody of the song of birds, so many and so sweet, and the shade of the palm trees, so tall and so beautiful, that the whole was a wonder. . . . As the number of islands in this region was so great that he could not give to each a separate name, the Admiral called them all by the common name of the Queen's Garden.

On the day following, the Admiral being very desirous to fall in with

[15] Dr. Chanca was a physician who accompanied Columbus on his second voyage, and who wrote back a long letter describing various natural objects in the New World, but saying nothing of the Remora.

some natives with whom he might parley, there came a canoe to hunt for fish:—for they call it hunting, and they hunt for one fish with others of a particular kind. They have certain fishes which they hold by a line fastened to their tails, and which are like conger-eels in shape, and have a large mouth [*i. e.*, head] completely covered with suckers, like the octopus. They are very fierce, like our ferrets, and when they are thrown into the water they fly to fasten themselves upon whatsoever fish they may espy, and sooner die than let go their hold till they are drawn out of the water.

The hunting fish is very light, and as soon as he has taken hold, the Indians draw him by the long cord attached to his body, and in this manner they take a fish each time on drawing both to the surface of the water.

As these hunters were at a distance from the caravel, the Admiral sent his boats to them with armed men, contriving it so that they should not escape to the land. As the boats came up to them, these hunters called out to the men in mildest manner and as unconcernedly as if they had known them all their lives, to hold off, because one of the fishes had fastened upon the under side of a large turtle and they must wait till they got it into the canoe. This our men did, and afterwards they took the canoe, and those in it, together with four turtles each of which was three feet in length, and brought them to the ships of the Admiral; and there they gave some account of these islands, and of their cacique who was close at hand, and had sent them to hunt. They asked the Admiral to go on shore, and they would make for him a great feast and would give him all of the four turtles they had caught.

Now for a short consideration of Peter Martyr's sources, which seem to be in common with those of Ferdinand Columbus, Las Casas and Bernaldez, if we may judge by the marked similarity of the accounts. There can be no doubt that Martyr, who during all the years of Columbus's voyages, was an attendant at the Spanish court, knew Columbus personally and held converse with him about his voyages and the wonders seen thereon. Winsor says (page 34) that "Peter Martyr knew Columbus," and adds that "Las Casas tells us how Peter Martyr got his accounts of the first discoveries directly from the lips of Columbus himself and from those who accompanied him." And on the next page (35) we read "Martyr . . . composed a special treatise on the discoveries in the New World . . . under the title 'De Orbe Novo' . . .

(which) occupied his attention . . . till the day of his death. For the earlier years he had . . . not a little help from Columbus himself."

Let us now see what Thacher, the latest and most profound of the biographers of Columbus, has to say as to Peter Martyr's sources, and we have done with this part of this paper. On p. 215 of volume II (1903), he says: "The Admiral and some of his followers wrote to Peter Martyr, and Peter Martyr thereupon wrote [a series of letters] to an Italian Duke and to a few Cardinals." On p. 218 ". . . Peter Martyr, who not only had access to all public documents, but who himself corresponded with Columbus." On p. 440 Thacher referring to Peter Martyr speaks again of ". . . Personal correspondence with the Admiral."

Confirmatory of all the preceding it may be noted that the Spanish Jesuit, Nieremberg, professor of physiology in the Royal Academy of Madrid, in writing of the Reversus, quotes Christopher Columbus. It seems not unlikely that he had in his day (his book was published in 1635) access to some of the Columbus manuscripts, may be to the journal of the second voyage. And earlier than Nieremberg, Gesner (1558) on page 483 refers to "Christ. Colũbus" as his authority for the story of the hunting fish. Furthermore Humboldt (1826) quotes Columbus on the activities of the *Reves*.

From a consideration of all this testimony, no other conclusion can be reached than that Peter Martyr had from Columbus's own lips or from his manuscript journal of the second voyage (see reference to Ferdinand's "Historie" on p. 447), or from both, the account of the use of the fisherman fish at the Queen's Gardens on May 19, 1494. Consequently the first man to see and describe the use of the sucking-fish as a living fish-hook was no other than Christopher Columbus, the great admiral of the ocean.

Long before this the reader has probably asked, "What belief is to be given these accounts of a matter apparently

so incredible?'' In answer first let us consider the innate probability of these accounts coming from such diverse sources. It hardly seems probable that such an extraordinary phenomenon, reported separately by Dampier, by Commerson, by Salt, by Holmwood and by Wills for one general locality, and by Columbus and his chroniclers for a part of the world nearly 5,000 miles away, could be other than an actuality. Indeed Humboldt, knowing only of Commerson's and the Spanish accounts, gave them full credence (1826 and 1833). He quotes Captains Rogers and Dampier, and Columbus, and then comments on the manner in which distant and alien peoples achieve the same ends by diverse means, the Americans having a fisherman-fish and the Chinese a fisherman-bird (the cormorant), both serving the same purpose. He thinks that the particular fish is not the small *Remora* but the large *Echeneis naucrates*.

P. H. Gosse (1851) in the volume on ''Fishes'' in his ''Natural History,'' refers briefly to the old use of Remora as a fisherman at ''Hispaniola and Jamaica'' and concludes as follows:

> From some observations of our own on the habits of a large West Indian[16] species, we are inclined to believe this account, though we do not know that the device is at present employed.

The distinguished Cuban ichthyologist, Felipe Poey (1856), refers to the Reversus story in a general way, does not seem to think it improbable, but is silent as to any such use in Cuban waters in his time, hence we may safely conclude that the Jardinellas de la Reina no longer witness the exploits of the fisherman fish.

But the reader may object that these stories, especially the Columbus accounts, date back into the far past, and may wish to know if there are any present-day statements to be adduced confirmatory of those already given. It may be answered that there is quite a number equally as

[16] Acting on this hint, Gosse's ''A Naturalist's Sojourn in Jamaica'' (1851) was carefully worked over, but with negative results.

circumstantial as those quoted above. These will be taken up chronologically for the localities involved.

And just at this point I am happy at being able to give what is almost an eye witness account of an almost present day use of the Remora as a living fish-hook in the very waters in which Columbus sailed. Lady Annie Brassey tells us that, while the "Sunbeam" lay at anchor in the roadstead of La Guayra, Venezuela in 1885:

> . . . in one of the Indian canoes which we passed we noticed a sort of sucking-fish (*Echeneis remora*), which is used in catching other fish. Arrived at the field of operations, the fisherman lets go an anchor and puts the sucking-fish, attached to a long line with a buoy at the end of it, overboard. It sees other fish at a great distance, darts after them, and attaches itself to them by means of the sucker on top of its head. The Indian easily raises his little anchor, paddles leisurely after the remora, removes the captured fish into his canoe, and repeats the operation until he has caught as many fish as he wants. Thus, one of the ugliest and most incapable-looking of creatures is made by savage instinct to become of some use in procuring food for the superior animal.

C. F. Holder, who knew the fishes of the Florida Reef as no other scientific man ever has, refers to Holmwood's accounts, makes mention of Columbus, notes that the fish is easily tamed and goes on to say (1905):

> It is this Remora of which the story is told that fishermen employ it in the Caribbean Sea to catch turtles. The Remora is kept, so runs the story, in a pail; a ring is placed about its tail and to this a line. When the men sight a turtle the Remora is slipped overboard and it is supposed darts at the turtle, seizes it, and holds on with such firmness and vigor that the animal can be hauled in.

It is interesting to note that in the first paragraph, Holder uses the present tense. Since he refers to them, he certainly had knowledge of the Columbus Guiacan stories in all of which the fish has no ring affixed to its tail, and is carried to the fishing grounds not in a pail but adhering to the outside of the canoe. The same account in almost the same words is found in one of the stories in his charming little book "Stories of Animal Life" (1899). In this the account of this curious fishing is somewhat amplified, and is accompanied by a drawing, Fig. 8, Plate III, of

this paper in a photographic reproduction of Holder's illustration and being a very spirited one is of interest and value. It is of course not a picture of an actual occurrence.[17]

In the paper previously referred to (1905) Holder tells of trying to catch turtles and sharks by means of a living fish-hook, in which effort, however, he was unsuccessful. He says:

> I experimented with the Remora but the fish invariably refused to dart after the turtle, preferring to find shelter under the boat. One tossed to a shark was seized by the latter, that doubtless thought it a votive offering. Possibly something was wrong: our remoras may have been stale: they surely were not ship or turtle slayers.

In this connection the only other modern figures of fishing with the living fish-hook may be given. Fig. 9, Plate III, is a reproduction of one of the illustrations from Hudson's "Curious Bread Winners of the Deep" (1893). It was made to illustrate the story copied from Ogilby's "America," and is reproduced here for the sake of completeness. The other figure number 10, Plate III, is from Frederic Ober's "Crusoe's Island" (1901). He gives the Columbus story and has had this figure drawn to illustrate it. The same data without the figure is found in an earlier book by Ober—"Travel Tales of the West Indies," 1888.

THE LIVING FISH-HOOK IN CHINESE WATERS

From the Caribbean we will go half way round the world to find the same story in all its essentials told of the fishermen along the southern coast of the Celestial Empire. Our reference here is to Frank T. Bullen, who in his delightful book "Denizens of the Deep" (1904) gives the following interesting account:

> Turtles are many on the Chinese Coast, and the guileful Chinese fisherman has developed a splendid plan for securing them with little

[17] The same figure and essentially the same data are to be found in Holder's "Half Hours with Fishes, Reptiles, and Birds." New York, 1906, page 80 and figure 49.

PLATE III

FIG. 8. Fishing with the living fish-hook. After Ober, 1901.
FIG. 9. Fishing with *Echeneis*. After Hudson, 1893.
FIG. 10. Fishing with the Remora. After Holder, 1899.
FIG. 11. An *Echeneis*, twenty-six and one half inches long, having a disk five and one half inches long, lifting a bucket of water weighing twenty-four pounds. After Townsend, 1915.

trouble to himself. He captures some *Remoræ,* those little sharks [?] that are so lazy that they have developed a sucking arrangement on the top of their heads, whereby they may, and do, attach themselves to anything that is likely to float them into the vicinity of food to be obtained without effort. Carefully he welds [?] a ring round their tails in such wise that it cannot be pulled off, and to it he attaches a thin, strong line; then, putting out to sea with six or seven of his unwilling helpers attached to the bottom of his sampan, he gets a good offing and waits patiently for the appearance of a turtle asleep upon the sea. As soon as his keen eyes have detected one, he paddles noiselessly in that direction until, getting near enough, he ships his paddle and, with a long bamboo, pushes off one or two or more of his *Remoræ.* Now all he needs to do is to keep them from fastening on to the canoe again, for they speedily discover the turtle and attach themselves to him. When they have done so, the quaint yellow fisherman in the boat needs but to haul in, for you may, by pulling upon a *Remora* from aft, tear him in two pieces, but you cannot make him let go his hold. And so despite his struggles the poor turtle must come [in]. . . .

In corroboration of this account, Dr. Alfred G. Mayor tells me that he has read in Singapore newspapers that the fishermen of that city commonly make use of the sucking-fish in the manner just described.

Fishing with the Remora in Torres Straits

There are now to be given a number of very circumstantial accounts of this mode of fishing in yet another part of the world—Torres Straits between Australia and New Guinea. The first is from the pen of John MacGillivray. In his "Narrative of the Voyage of the Rattlesnake" (1852), volume I, page 300, he tells of the rescue of a white woman, Barbara Thompson by name, who had been held captive for some years by the natives of Muralug or Western Prince of Wales Island in Torres Straits and had been named by them Giom or Gi(a)om.

In Volume II, pages 21–22, MacGillivray says:

This last (an unnamed species of turtle), I was informed by Gi'om, is fished for in the following extraordinary manner. A live sucking-fish (*Echeneis remora*), having previously been secured by a line passed round the tail, is thrown into the water in certain places known to be suitable for the purpose; the fish while swimming about makes fast by

its sucker to any turtle of this small kind which it may chance to encounter, and both are hauled in together.

Our next account is an eye witness one dated but a few years after MacGillivray's. John Jardine was for some years police magistrate at Somerset, Cape York, where his duties brought him into close contact with the natives. As a result of his experiences, in 1866 he published the following account of fishing with the sucking-fish at Cape York:

A singular mode of taking the hawkbill turtle is followed by the natives here. This custom, though said to be known so long back as the time of the discovery of America by Columbus, is so strangely interesting that I will give a short account of it as I have seen it practised. A species of sucking-fish (Remora) is used. On the occasion to which I allude, two of these were caught by the blacks in the small pools in a coral reef, care being taken not to injure them. They were laid in the bottom of a canoe, and covered over with sea-weed—a strong fishing-line having been previously fastened to the tail of each. Four men went in the canoe; one steering with a paddle in the stern, one paddling on either side, and one in the fore-part, looking out for the turtle and attending to the fishing lines; while I sat on a sort of stage fixed midship, supported by the outrigger-poles. The day was very calm and warm, and the canoe was allowed to drift with the current, which runs very strong on these shores. A small turtle was seen, and the sucking-fish was put into the water. At first it swam lazily about, apparently recovering the strength which it had lost by removal from its native element; but presently it swam slowly in the direction of the turtle, till out of sight; in a very short time the line was rapidly carried out, there was a jerk, and the turtle was fast. The line was handled gently for two or three minutes, the steersman causing the canoe to follow the course of the turtle with great dexerity. It was soon exhausted and hauled up to the canoe. It was a small turtle, weighing a little under 40 lbs., but the sucking-fish adhered so tenaciously to it, as to raise it from the ground, when held up by the tail, and this some time after being taken out of the water. A strong breeze coming on, the canoe had to seek the shore without any more sport. I have seen turtles weighing more than 100 lbs., which have been taken in the manner described.

We next hear of this fish in Gill's "Life in the Southern Isles" (1876), wherein he corroborates MacGillivray and Jardine in the following citation:

Another mode of turtling is to call in the aid of the *Echeneis remora,* or sucking-fish, which is about three feet in length, and is easily caught by a line. When caught the Straits Islanders pierce the tail, in order to insert a strong cord, which is also wound round it for the sake of security. Generally captive sucking-fish are kept swimming after the canoe until a turtle is seen, when three or four of them are thrown as near the sleeper as possible. These sucking-fishes at once attach themselves to the turtle, which awakes to find itself a prisoner. The cords are now cautiously hauled in, bringing the sucking-fishes and the turtle. This ingenious device is used only with the smaller turtle. Sucking-fishes are sometimes kept two or three days in a lagoon or in a boat half-filled with sea-water, until turtles are seen.

In 1888, Professor A. C. Haddon was a member of an expedition to Torres Straits to study corals, and while there (some eight months) he made notes of the use of Echeneis as a turtle-catcher and of its supernatural powers. Brief accounts of this remarkable use of the fish were published in 1889, 1890 and 1890a (see in bibliography under Haddon), but as a much fuller account by him will be given later the above need not be quoted here.

Stirred up by Haddon's note of 1889, Sclater later in the same year in *Nature* called attention to Holmwood's article. And, stirred by Sclater, H. Ling Roth in the same volume of the same journal cited the account by Ferdinand Columbus given in Churchill's Voyages as quoted on page 448.

Saville Kent in his book, "The Great Barrier Reef of Australia" (1893), has the following to say anent our subject:

A method frequently employed by the natives of Torres Straits to capture turtles is remarkable. The large sucking-fish, *Echeneis naucrates,* which grows to a length of three or four feet, and is distinguished by the natives by the title of "Gapu," is pressed into service. The fish is kept alive in water in the bottom of the native canoe, a thin line being fastened round its tail and through its gills. On a turtle being sighted in the vicinity of the canoe, the sucking-fish is thrown towards it, and immediately swims to and fastens on its carapace. If the turtle is of small or medium size, it is hauled in by the line, the fish retaining its tenacious hold; but if it be a large one, a native jumps overboard with a stronger line, and, following the smaller one down, secures the reptile.

Corroboratory of the foregoing is the following account extracted from Semon's book "In the Australian Bush" (1899). In describing the catching of the turtle, *Chelone midas,* by divers who jump on its back, or by fishermen who harpoon it, Semon adds:

> . . . but a third very peculiar method of capture is adopted in Torres Straits. In clear weather and a tranquil sea, the sharp eye of the native is able to discern any turtle reposing on the bottom of the sea in the neighborhood of the coral reefs. Now a sucking-fish, or Echeneis, to the hind fin of which a long string has been fixed, is thrown into the water above the place where the turtle has been seen. It will immediately descend into the depth and attach itself to the shell of the reposing Chelonian, and as a communication is thus established between the boat and the turtle, a native following the leading string, dives and winds a rope round the beast, as the sucking-fish does not attach itself quite firmly enough for the fisherman to draw the heavy weight up by it.[18]

This last statement must not be interpreted as contradictory of the foregoing accounts of catching turtles *on the surface* with the sucking fish. Bringing boat and turtle together on the surface by pulling on the line is one thing, hauling a turtle up from the bottom is quite another, as any reader knows who has ever endeavored to land a ray or other large flat fish which insisted on clinging to the bottom. This latter is purely a problem in hydrostatics.

Entirely independent of any of the foregoing accounts is that of the Australian ethnologist, W. E. Roth. Here the location (Tulley River) is different, as is the final manner of taking the fish, turtle, or dugong. Roth's statement follows:

> On the coast-line in the neighborhood of the Tulley River, the sucker-fish, *Remora,* is utilised as a guide for spearing or harpooning fish, as well as turtle and dugong. This sucker-fish, known to the Mallanpara blacks as *kamai,* is found usually on the rocks at the outlying islands, and sometimes stuck on their own canoes. It is removed, kept in a canoe, bark-trough, etc., with a little water, and left there for a few days. Then, going out to sea, the native ties a fine twine round the Remora's tail, and as soon as he sights any big fish, turtle or dugong,

[18] This account is also found in the German edition of Semon's book published at Leipzig in 1903.

advances his canoe as far as possible, and drops the sucker-fish overboard. In all probability, the sucker will go straight for the object and attach itself: it acts only as a guide, and tells the hunter the next move of his prey. The aboriginal now plays the line out very guardedly, draws it in with equal care and caution, and as soon as the length submerged reaches a point on the line, previously marked, he knows that he is within striking distance, and as his quarry comes to the surface, uses the spear or harpoon accordingly. It must be borne in mind that in no sense does the sucker-fish pull the prey into the hands of the hunter: it only indicates the direction in which the harpoon, etc., can be advantageously thrown.

The account given by N. W. Thomas in his book "Natives of Australia" (1906) is taken almost verbatim from the above and beyond this mere citation no notice will be taken of it here.

We now come to another account of the peculiar use of *Echeneis* under discussion, and I am able to offer no less an authority than the "Encyclopædia Britannica," in the eleventh edition of which, in Volume XXII (1911), in the article on Queensland, Australia, Mr. T. A. Coghlan writes:

In Torres Strait and the northern coast the hawksbill turtle . . . is said to be captured in a peculiar manner, the sucking-fish or remora (*Echeneis naucrates*) being utilized by the islanders for that purpose. The remora is carried alive in the bottom of the canoe, a long thin line being attached to the fish's tail and another usually to the gills. On a turtle being sighted and approached to within the length of the line, the sucking-fish is thrown towards it, and immediately swims to and attaches itself by its singular head sucker to the under surface of the turtle which if of moderate size is easily pulled into the canoe.

During the year 1898, Professor A. C. Haddon was leader of the Cambridge University Anthropological Expedition to Torres Straits. On this expedition he made an extensive study of the use of the fisherman fish. Professor Haddon's data is so complete that he has effectually settled the matter of the present-day actual use of the fish for taking other fish, and since his reports are of the highest value, putting as they do the imprimatur of truth on the whole matter, they will be referred to in some detail.

Professor Haddon's first account based on the data of

his second expedition is to be found in his "Head Hunters: Black, White and Brown" (1901). This gives essentially the same data as that contained in the short article in *Folklore,* 1890, but for fuller accounts we must turn to the various reports of the Cambridge Anthropological Expedition to Torres Straits.

Taking up these reports chronologically fits in well with the scheme of this paper, as will be seen presently. Volume V (1904) deals with the "Sociology, Magic and Religion of the Western Islanders." Here Haddon gives three folk tales, one of which has to do with the origin of the use of the *Gapu* (the native name of the sucking-fish), and two with its use. Later in the same volume Dr. Rivers gives a very detailed account of the method of procedure in fishing with the *Gapu.* This data will be found later in Haddon's final account of the use of this fish. Further along in Volume V Haddon and Rivers give accounts of the *Gapu* as a totem.

Volume VI of the Reports bearing date 1908 has for its title the "Sociology, Magic and Relation of the Eastern Islanders." These peoples do not seem to have so many tales of the *Gapu* as their western brethren since Haddon records but two. It seems apart from the purpose of this paper to insert any of these folk tales here, but it is my purpose later with Professor Haddon's kind permission to collect them and publish them in a short article.

We now come to the latest, most detailed, and most valuable of all the accounts of the use of the living fish hook in Torres Straits. In Volume IV of the Reports issued in 1912, Professor Haddon gives a very circumstantial account and this will be quoted in full. In this volume, dealing with arts and crafts, fishing with the sucking-fish is frequently referred to. The fish is well known to the natives as their myths and legends show and it is a common motif in their ornaments and ornamentation. Haddon's account of its use now follows:

> The most interesting method of catching turtle is that in which the sucking fish (called *gapu* in the western part of the straits, and *gep* in

the eastern) is employed. . . . The sucker-fish is not used to haul in the large green turtles; I was repeatedly assured that it would be pulled off, as the turtle was too heavy; but small ones are caught in this manner. . . .

According to one of the folk tales, there was a time when the people of Badu did not know how to catch turtle by means of the sucker-fish, and they used to employ a black toothless "dog-fish," *Kumsar,* when they went for turtle. The story goes on to tell how Bia taught his fellow islanders how to employ the sucker-fish. In the Bomai-malu legend of the Miriam, it is stated that Barat of Moa, according to the fashion of olden times, tied a rope around the tail of a *kamosar,* then he made a sucker-fish, and instructed the Western Islanders who were with him how to catch turtle with it. I do not understand how turtle could be caught by a "dog-fish," but as the identity of this fish, which is said to live in the crevices of the rock in deep water, is unknown, nothing further can be said, except to hazard the suggestion that it may be an unidentified kind of lamprey; but against this it must be stated that no member of the Cyclostomata is known from Queensland waters, though *Mordacia mordax* occurs in Tasmania and species of *Geotria* are found in southern Australian waters.

I was informed that in leashing a sucker-fish, a hole is made at the base of the tail-fin by means of a turtle-bone and one end of a very long piece of string inserted through the hole and made fast to the tail, the other end being permanently retained. A short piece of string is passed through the mouth and out at the gills, thus securing the head end. By means of these two strings the fish is retained, while slung over the sides of the canoe, in the water. The short piece is pulled out of the mouth of the fish when the turtle is sighted and the *gapu* is free to attach itself to the turtle.

According to Professor Haddon there is a certain ceremonial or set rule of procedure always definitely followed in fishing with the *gapu.* This he describes as follows, his data being chiefly taken from Rivers as noted above:

When starting on a trip to fish for turtle by means of the sucker-fish, the owner (or captain) of the canoe gives the order where to go and when to let go the anchor, having arrived at their destination.

The *buai-garka* (mate, also brother-in-law of the owner or captain) makes a fire on which he places some turtle-bone which the owner has brought with him. When the bone is charred the *buai-garka* breaks it up and throws it into the water so as to attract the sucker-fish. When one is caught it is the duty of the *buai-garka* to attach to the fish the leashing which he had previously made.

The direction of affairs is now assumed by the *buai-garka,* who gives the word to move to another place, and the directions where to go.

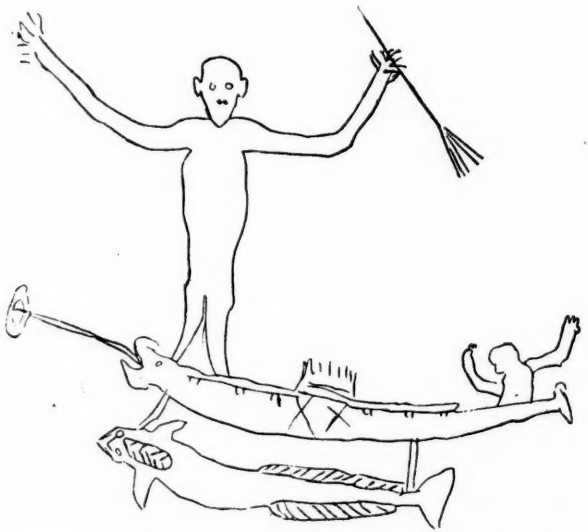
When he gives the order to stop, the mat sail is rolled up by the other men (or at the present time the sail is lowered), he not taking any part. He gives the order to paddle till he sees the turtle, then gives the word to stop, and the anchor is let go by the owner, having been previously shifted to the stern of the canoe. When the *buai-garka* sights a turtle swimming deep down in the water, he removes the mouth string from the sucker-fish and throws the fish overboard with the tail-line attached and plenty of slack is thrown with it, he then hauls in the superfluous slack and as far as possible indicates the direction of the turtle by pressure on the line. The sucker-fish on perceiving the turtle immediately swims towards it, and attaches itself to the reptile's carapace. When this is accomplished, the *buai-garka* gives the order to heave up the anchor and move the boat up to the position of the turtle.

One of the crew (but not the *buai-garka*), with a long rope attached to the right upper arm, dives into the water, and is guided to the turtle by the line fastened to the fish's tail. On reaching the turtle, the man gets on to its back and passes his arms behind and below the fore flappers and his legs in front of and below the hind flappers, or secures it in some other way. The man is then rapidly drawn up to the surface of the water bearing the turtle with him. On the arrival of the diver the sucker-fish usually shifts its position from the upper to the under surface of the turtle. As soon as enough turtle have been obtained, the owner of the canoe gives the order to go home, and the *buai-garka* resumes his subordinate functions, and resigns into the hands of his brother-in-law the direction of affairs which had been his part during the actual process of fishing. The *buai-garka* knows whether the fish has attached itself to a turtle or to a shark by the nature of the motion of the string. If the pull is intermittent it means that the fish has adhered to a shark, but if steady, then a turtle has been secured.

In a footnote Professor Haddon tells us that the sucker-fish is eaten at the end of the day's fishing. This seems like a very wasteful course of action, but it may have arisen because of the difficulty in keeping the fish alive until the next time it would be needed. In text-figure 5, we have a native drawing showing how the "Gapu" is attached to the canoe during the trip to the fishing grounds.

We have in Holmwood's account a description with figures of how the leashing is accomplished, and Haddon also is too good a scientist to leave us in doubt as to how the Torres Straits natives manage this matter. In addition to what has already been given as to the manner of making the leashings fast, his detailed account is as follows:

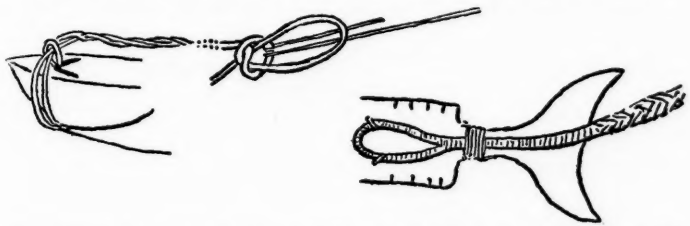
In order to understand the method of leashing a sucker-fish, I induced a native to make a model of a *gapu* for me. Fig. 173 [present text-figure 5] indicates diagrammatically the arrangements. A loop is inserted by means of a wooden arrow point through the gills and out at the mouth, the ends are passed through the loop, and one of the strands is threaded through the other, the two are then twisted into a string. The mouth string is called *gudaz* and is made of the inner bark of the root of the *wali* tree. The other end of the *gudaz* is tied into a slip knot, *kaza wiaikab,* the end of a long piece of twine is simply bent twice round the string at the knot; when the free end of the *gudaz* is pulled



TEXT-FIGURE 5. Showing how the sucker-fish is leashed by the natives of Torres Straits. Drawing of Tagai and Kareg in their canoe by Gizu of Mabuiag, reduced by one half. In this drawing, the canoe, Kareg and the sucker-fish are represented the wrong way round. After Haddon, 1912.

the knot runs out, and the twine (one end of which is still held, by the fisherman) slips off the *gudaz*. The main fishing string is a very long and strong cord of twisted coco-nut fibre, *igal;* this is fastened to a braid of plaited string, *dan,* the other end of which is bent round on itself so as to form a loop; the end of the *dan* and the loop are bound round with *wali*. The loop is furnished with two strings of *wali;* it looks as if these were threaded through the tail of the fish above and below the vertebral column and tied together on the other side. Another lashing binds the cord close to the side of the narrow portion of the tail.

The sucker-fish is so well known to the natives as to give rise to a decorative motive in their decorative art. Haddon gives numerous figures of this. It is also a subsidiary totem of one of the clans in the western islands and Haddon thinks may once have been a chief totem of a



TEXT-FIGURE 6. Native drawing illustrating the method of attachment of the sucker-fish to the canoe in Torres Straits.

larger but now extinct clan. The natives moreover ascribe to it considerable intelligence. Haddon thus concludes his interesting account:

> The natives have great respect for the sucker-fish and firmly believe it to possess ominous powers. For example: when the fish does not take a good hold on the turtle and then swims off it indicates that some part of the canoe is not secure; when there is something the matter with the bow of the canoe, the fish is said to attach itself to the neck of the turtle, but should the stern of the canoe be weak, the fish adheres to the extreme hinder end of the carapace; when it fixes itself firmly to the front part of the carapace, the canoe is strong; when it goes to one side of the carapace or keeps moving about, it shows that the leashings of the float to the outrigger on that particular side are insecure. More than once I was told, *Gapu* savey all same man. I think him half devil (*i. e.*, spirit).[19]

One other account is now to follow and all the known data will have been fully presented to the reader. E. J. Banfield, the well-known Australian, lived for a number of years on Dunk Island, off the coast of tropical Queensland, in about latitude 18° S. His experiences there are

[19] After Haddon's full and very detailed accounts, Meek's brief reference (1913) that the ingenious natives of Thursday Island and the adjacent parts of Torres Straits use one fish to catch another—*i. e.*, the sucking-fish with a string fastened to itstail—needs no fuller statement than that given in this footnote.

recounted in a charming book, "The Confessions of a Beachcomber." This was first published in 1908, and reprinted in 1910, and again in 1913. The following quotations are from the 1913 reprint:

Generally unprogressive and uninventive, the aboriginals of the coast of North Queensland apply practically the result of the observation of a certain fact in the life history of a fish to obtain food. By them the sucker (remora) is not regarded as an interesting example of a fish . . . , but as a ready means of effecting the capture of . . . two . . . animals (turtle and dugong), always eagerly hunted for their flesh.

Other countries have sucker-fish of different form; but it remained for the benighted Australian blacks, among a few other savage races, to make practical use of the creature, which, as a means of locomotion, forms strong attachments to the dugong, turtle, shark and porpoise. It can hardly be called domesticated, yet it is employed after the manner of the falcon in hawking, save that the sucker is fastened to a light line when the game is revealed.

Having located the haunts of a remora the blacks feed it from day to day until its shyness is worn off, and then catch it with a hook.

Having secured the sucker, the blacks farm it in their haphazard fashion. They fasten a line above the forked tail so securely that it cannot slip, nor be likely to readily cut through the skin, and tether it in shallow water, when it usually attaches itself to the bottom of the canoe. When as the result of frequent use and heavy strain, the tail of the sucker is so deeply cut by the line that it is in danger of being completely severed, a hole is callously bored right through the body beside the backbone, and the line passed through it for additional security.

When ready to hunt for turtle, the natives armed with spears go out in their bark canoe to the bottom of which one of the sucking fish is attached by its sucker. When they reach a locality where turtle abound, they soon get into action.

In sight of the game the sucker which has been adhering to the bottom of the canoe is tugged off and thrown in its direction. As a preliminary the disc and shoulders of the sucker are rigorously scrubbed with dry sand or the palm of the hand, to remove the slime and to excite the ruling passion of the fish. It makes a dash for a more congenial companionship than an insipid canoe. The line by which it is secured is made from the bark of the "Boo-bah" (*Ficus fasciculata*) and is of

two strands, so light as not to seriously encumber the sucker, and yet strong enough to withstand a considerable strain. Two small loops are made in the line at intervals of two fathoms from the sucker, to act as indicators.

As soon as the sucker has attached itself to the turtle, a slight pull is given and the startled turtle makes a rush, the line being eased out smartly. Then sport of the kind that a salmon-fisher enjoys when he has hooked a 40-pounder begins. The turtle goes as he please; but when he begins to tire, he finds that there is a certain check upon him—slow, steady, never-ceasing. After ten minutes or so a critical phase of the sport occurs. The turtle bobs up to the surface for a gulp of air, and should he catch sight of the occupants of the canoe, his start and sudden descent may result in such a severe tug that the sucker may be divorced. But the blacks watch, and in their experience judge to a nicety when and where the turtle may rise; telegrams along the line from the sucker give precise information. They crouch low on their knees in the canoe, as the game emerges with half-shut eyes and dives again without having ascertained the cause of the trifling annoyance to which he is being subjected. The line is shortened up. Perhaps the turtle sulks among the rocks and coral, and endeavors to free himself from the sucker by rubbing against the boulders. Knowing all the wiles and manœuvres, the blacks play the game accordingly, and hour after hour may pass, they giving and taking line with fine skill and the utmost patience. The turtle has become accustomed to the incumbrance and visits the surface oftener for air. One of the harpoons is raised, and as the turtle gleams grey, a couple of fathoms or so under the water, the canoe is smartly paddled towards the spot whence it will emerge, and before it can get a mouthful of air the barbed point, with a strong line attached, is sticking a couple of inches deep in its shoulder.

From the foregoing interesting accounts it is clear first that in Torres Straits at the present time the aborigines use the sucking fish as a living fish-hook just as fishermen in other regions use a veritable fish-hook, *i. e.*, to bring the fish or turtle to the gaff. The gaff is, in the last account quoted, a harpoon or spear, according to others a native who dives down, guided to the turtle by the line attached to the Echeneis, and who then ties a line to a flipper of the turtle.

THE ENGLISH SPARROW HAS ARRIVED IN DEATH VALLEY: AN EXPERIMENT IN NATURE[1]

THE English sparrow first became well established in the United States in 1860–1864 in the vicinity of New York City. Several small plants had been made in other Atlantic cities within the few years preceding, but practically all of these are definitely known to have failed. The original stock is in nearly all the cases of importation known to have been obtained in England. Its spread through the eastern United States after once established was phenomenal; its rate of invasion towards the west only slowed up at about the 100th meridian, and this, significantly enough, is about at the line limiting a great many species of native eastern birds toward the west and of native western birds toward the east. Nevertheless, the English sparrow ultimately crossed this barrier, constituted by change in humidity, and it has continued expanding its range until it exists now in nearly every part of every state in the Union. It has also extended throughout southern Canada and has become well settled in the Hawaiian Islands.

In California the English sparrow was first noticed in 1871 or 1872, in San Francisco, and it quickly thereafter appeared in many of the towns in the west-central part of the state. But it was very slow to enter southern California. It did not reach Los Angeles for nearly thirty-five years, in 1907; and San Diego was not reached until 1913. To-day it is familiar in practically every town "south of Tehachapi." Among the places in California now inhabited by the English sparrow, to designate some of those showing extremes of climate as regards temperature and humidity, are Brawley, Imperial County, and Sisson, Siskiyou County; Needles, San Bernardino County, and Eureka, Humboldt County.

In 1917 the California Museum of Vertebrate Zoology undertook as field work for that year a study of the vertebrate animal life of the Inyo region of southeastern California. In connection with this work it was the writer's not unpleasant fortune to spend the month of April in Death Valley. What was his sur-

[1] Contribution from the Museum of Vertebrate Zoology of the University of California.

prise to find there a thriving colony of English sparrows. These were established on the Greenland Ranch (otherwise known as Furnace Creek Ranch), elevation 178 feet *below* sea level. Specimens were collected, both as alcoholics and as dry study skins, but not to an extent to threaten the persistence of the colony. For here, it occurred to the writer, we had at hand a particularly convincing "experiment" already under way, of just the sort called for by certain critics of the work of the systematist and distributionist, which in time would test the question of the evanescence versus the relative permanence of characters of the category commonly viewed as subspecific.

The sparrows of Furnace Creek Ranch, which were estimated to number about fifty, had their main headquarters in the tops of the several tall Washington Palms which overshadow the ranch house; also several nests were seen in the Fremont cottonwoods which line the irrigation ditches along the alfalfa fields for a quarter of a mile down toward the glistening borax flats. The traveller on entering Death Valley is impressed by Greenland Ranch as a wonderfully rich oasis surrounded by a desert of surpassing barrenness. The English sparrow colony there is, then, isolated under a climate that is probably of the greatest extreme in the direction of high temperature combined with low relative humidity, of any place in North America.

Greenland Ranch is owned by the Pacific Coast Borax Company, who value it for its output of alfalfa hay and for certain appurtenant water rights, there being a constant flow of forty inches from the warm springs nearby. Fortunately for our present problem, the company has for years required its managers to keep a daily record of weather conditions. There is a standard instrument shelter, and the records are kept in available form, and furthermore have been transmitted regularly to the United States Weather Bureau. Without going into details here, it is of interest to note that the highest recorded temperature for any place in the United States was observed there on July 10, 1913, when an afternoon temperature of 134° Fahrenheit in the shade was reached.

As to the time of appearance of the sparrows in Death Valley I have good reason to rely on the statements of Mr. Oscar Denton, who is the present manager of the Greenland Ranch. He says that he first saw them in the ranch yard five years ago (1914). That was about the time the Death Valley spur of the Tonopah and Tidewater Railroad was run to the present location

of Ryan. Ryan, by the way, is the terminus of the narrow-gauge line, wherever that terminus happens to be, and this shifts about as determined by the extent of the different ledges of borax ore mined. The borax deposits on the floor of Death Valley are no longer gathered. The day of the 20-mule-team borax wagons is gone except on the labels. It is cheaper to handle the richer borax ore high on the mountain sides and to reach these ledges by railroad. The present Ryan, the nearest the railroad has so far gotten to Death Valley, is 17 miles from Greenland Ranch and 3,000 feet altitudinally above it. I saw English sparrows there repeatedly in April and May, 1917, as also at Death Valley Junction, 40 miles farther away, on the Tonopah and Tidewater Railway. Mr. Denton believes, and I think he is likely right, that the sparrows followed the construction camps along the route of the T. & T. R. R. from Ludlow to Death Valley Junction and thence along the narrow-gauge to Ryan. It may be further suggested that since hauling is done from time to time down the 17 miles of Furnace Creek Wash from Ryan to Greenland Ranch this is the route probably travelled by those sparrows which reached Death Valley. It is less probable to my mind that the birds simply started out overland, from some more distant point, and a pair or more just happened to reach this remote and forbidding valley. It is true, however, that the green of the ranch shines out conspicuously for miles round about and would surely attract to it any vagrant sparrow coming within sight.

We here in America have been accustomed to think of the English sparrow as a full *species, Passer domesticus*. The bird was originally named by Linnaeus, and thus has seemed from all standpoints to constitute a truly "Linnaean species." However, recent developments in the geographic knowledge of birds in the Old World has brought out the fact of geographic variation within the species *Passer domesticus* as previously understood, and also that a number of forms once considered specifically distinct are really connected with the *domesticus* stock through ordinary geographic intergradation. Hartert (1910, pp. 147–151) after a study of the group came to recognize no less than eight subspecies occupying different areas in Europe, western Asia and northern Africa. Subsequently, at least two more races have been named. And now, a German, Kleinschmidt, has discovered that the sparrows of England are distinguishable from those on the continent. The latter, having been

the basis of Linnaeus's name, becomes *Passer domesticus domesticus,* and the sparrow of England Kleinschmidt names (cited under date 1915, though I have not seen the original description myself) *Passer domesticus hostilis.* As pointed out by Oberholser (1917, p. 329), since the American stock came from England our bird must also be known under this name. And furthermore, the vernacular term, European house sparrow, which some people have preferred because of a fancied unpleasant association in the name English sparrow, can not be used properly for the American bird.

The point I wish to make now is that the English sparrow, which is spread all over the United States, is itself a subspecies of a wide ranging and decidedly variable species which is thus, geographically speaking, quite like our American song sparrow, or the horned lark. In the Old World, each race "stays put" as regards aggregate of population, each in its own faunal area just as do our own song sparrows. All of these races are nonmigratory. *Passer domesticus hostilis* Kleinschmidt is also nonmigratory, as far as I have been able to learn, wherever it now occurs, north and south, in America. But here, by reason of its marvellous powers of accommodation, and finding no competitor in exactly its own ecologic niche, it has gradually advanced its frontiers and overleaped all the faunal boundaries which hem in the habitats of our native bird races; and we find flourishing representations of it under the most diverse conditions of environment, as for example those shown in contrast by Death Valley and Boston.

Possibly our critics have been merely baiting us when they asked us to transplant a desert song sparrow to the humid coast belt and "see what would happen." But is not this demand met exactly in the case of the English sparrow, only in reverse direction? I have carefully compared the seven skins taken in Death Valley with others taken in Berkeley, and also with examples taken in the eastern United States, without finding any peculiarities of color tone, extent of markings, or dimensions. And I think my eyes are pretty well trained to find small subspecific distinctions, at least of such magnitude as characterize the currently recognized subspecies of song sparrows, Savannah sparrows, and horned larks. The Death Valley birds, it is true, stand out rather sharply from most of the material taken elsewhere, but only in that they are fresh and clean, and lack the sooty overcast of the majority of town birds. To repeat, no dif-

ferences are now discernible from place to place in North America, in so far as perfectly comparable material is at hand. This accords with the findings of Phillips (1915), which also were practically negative.

Are we not to infer, then, that there has not as yet been sufficient time (up to three years and as many possible generations in Death Valley and up to sixty years elsewhere in North America) for the impress of diverse environments in the different parts of the territory newly occupied by *Passer domesticus hostilis* to bring physical changes in the birds of sufficient magnitude for the modern systematist to detect? Is there not here a demonstration of the relative permanence of subspecific characters far beyond what many naturalists have supposed? Are not such characters in general far more likely to be germinal than somatic?

How intensely interesting it will be to watch the course of this "experiment," now under way, irrespective of human effort, in Death Valley, with "controls" vigorously maintaining themselves (against man's wish!) in San Diego, Berkeley and Boston.

But perhaps it will be urged that the conditions of an orthodox experiment are not here properly met. The "factors" of the environment are not sorted out, and none is under any kind of regulation. Moreover, rigid control has not been secured, in that there is no way in which any of the naturally established colonies of English sparrows can be strictly isolated and kept from genetic contamination by new influxes of birds from elsewhere.

In reply, I would say that we are not expecting more from our natural experiment than the demonstration of what we set out to prove, namely, the length of time necessary for the development, in a stock under natural conditions some of which are known, of characters of subspecific value. In the breeding cage there are always "unknown" factors; so let us admit the existence of those in the wild as not invalidating the "experiment" as such. In nature, subspecies *have* differentiated under just the conditions self-imposed by the English sparrows through their powers of invasion. *Individual* song sparrows and horned larks are continually overstepping the bounds of the habitats of the races to which they belong and have doubtless done so since the initiation of their respective descent lines. But differentiation of the mass *has taken place, under just these conditions.*

JOSEPH GRINNELL